THURSDAY, OCTOBER 1, 1896.

CHEMISTRY IN DAILY LIFE.

Chemistry in Daily Life: Popular Lectures. By Dr. Lassar-Cohn. Translated by M. M. Pattison Muir. Pp. x + 324. (London: Grevel and Co., 1896.)

A BOOK which professes to instruct the public, uninitiated into technical language or methods, concerning the results of the application of scientific principles to the purposes of daily life, must possess a combination of qualities not easily associated together. It ought to be true—that is, the positive statements it contains ought to be facts, and yet, though its pages should present the truth and nothing but the truth, it is impossible that it should give the whole truth in regard to many subjects it must pretend to discuss. Here is the grand opportunity for the exercise of judgment on the part of the writer, without which and a large proportion of sympathy with his readers the book will be both unintelligible and uninteresting. There must be—and there are—many subjects which, from their nature, are incommunicable to the mind not already prepared with a knowledge of fundamental ideas and some familiarity with the technical language or symbols by which these ideas are expressed. Such subjects as many divisions of pure mathematics and, we will venture to add, of modern chemistry belong to this category.

However, acting upon the view that the best test of the suitability of such a book for the general reader is not merely the opinion of the chemical expert on the subject-matter and the degree of accuracy of the notions introduced, the writer of this notice has placed this little volume in the hands of an educated but not technically instructed friend, with a request to read it carefully, appealing for help or explanation if necessary. This is the kind of thing that follows :—

"Please tell me the meaning of this : 'The green parts of the leaves are called chlorophyll-grains,' also 'silica is the chemical name for pure sand'; and, pray, what is humus?" (pp. 38-40).

A little later the reader says :

"Listen : 'A cannon exhibited by Krupp at the Chicago Exhibition, when charged with 115 kilos of this powder, propelled a shot weighing 215 kilos to a distance of 20,226 metres ; the flight of the shot occupied 70 seconds, and the highest point attained was 6540 metres above the earth, while the height of Chimborazo is only 6421 metres.' What does all that mean, and what has the highest point got to do with it?"

These are sufficient examples of the, perhaps, not very serious difficulties encountered by the general reader, who at the end remarked, "Oh, yes; I found it interesting."

Now let the chemist take a look at the volume. As already hinted, the impossibility of stating some things without resort to technical language leads to a great deal of extremely loose and objectionable phraseology. Take the following passage (p. 46) for example :—

"Most of the phosphoric acid in the materials we have mentioned is combined with lime in the proportion of three molecules of lime to one molecule of the acid. Sulphuric acid is a stronger acid than phosphoric; but one molecule of sulphuric acid combines with only one molecule of lime. If then two molecules of sulphuric acid are caused to react with burnt bones or mineral phosphorite, a new compound is obtained, in which one molecule of phosphoric acid is combined with one molecule of lime, and, at the same time, two molecules of sulphate of lime or *gypsum*, as it is commonly called, are formed. The following scheme makes the process more evident.

$$\text{Phosphoric Acid} \begin{matrix} \text{Lime} \\ \text{Lime} \\ \text{Lime} \end{matrix} + \begin{matrix} \text{Sulphuric Acid} \\ \text{Sulphuric Acid} \end{matrix}$$

Here we have a series of statements all more or less open to criticism, the culminating misrepresentation being embodied in the scheme, which asserts that sulphuric acid withdraws lime from the phosphate without leaving anything in the place of it. This, however, is just the kind of thing which it is well-nigh impossible to express correctly in popular language. The worst of it is that the same erroneous idea crops up in so many other places. The worst case we have encountered occurs on p. 51, where ammonia is said to be "an alkali or a base, for these names have to-day the same meaning." And a few lines further on it is announced that "bases and acids may be gases, liquids, or solids. Ammonia, for instance, is a basic gas, carbonic acid is an acid gas, sulphuric acid is a liquid, and silicic acid is a solid." After such a descent towards the popular level, it is difficult to believe that anything can be gained by the introduction of chemical formulæ, especially such as occupy the last ten pages, where an attempt is made to explain the constitution of alkaloids and other complex carbon compounds.

All this kind of thing was managed much more successfully in "The New Chemistry" of the late Prof. Josiah P. Cooke, which, though published twenty years ago, is still trustworthy and, in point of literary quality, incomparably superior to such a jumble of information not always to be depended upon for accuracy, and sometimes descending to the almost ludicrous. One cannot but wonder whether the author was serious or cynical when he wrote that phosphoric pig-iron "is only fitted for making the coarsest sorts of cast-iron ware, such as *railings for graves* and the like, *in which no great durability is looked for.*" The italics are ours.

The author does not often exhibit emotion, but bimetallism is too much for him, and he lets his pen run. The whole story is too long to quote, but one passage affords such a remarkable example of style, unspoiled by a conscientious translator, that it is worth reproducing.

"There is one thing which the bimetallists would certainly achieve, as long as they do not get rid of the fluctuations in the price of silver, were they to induce the civilised States to inaugurate an international bimetallism in that Utopia which they depict to any one who will hearken to them as the approaching economical rejuvenescence of the nations—for none of them has brought forward a decisive argument in favour of their assertions because no such argument exists, for if there were such an argument it would certainly be easy to induce the most influential nations to adopt bimetallism again—and this one thing which they would undoubtedly do would be to enable the proprietors of American and Australian silver mines, one of whom is already the richest man in the world, to make yet much greater

profits from their mines, in which profits Europeans have as yet no great interests," &c.

After this it is not surprising, as the translator informs us in the preface, that the publication of the book "caused quite a stir in German circles."

OUR BOOK SHELF.

Crystallography for Beginners, with an Appendix on the use of the Blowpipe and the Determination of Common Minerals (after the method of Dr. Albin Weisbach). By C. J. Woodward, B.Sc. Pp. 164. (London: Simpkin, Marshall, Hamilton, Kent and Co., Ltd., 1896.)

IN a preliminary chapter of this book the student is taught how to prepare for himself, with due regard to economy of purse, a set of models to be used in connection with the various lessons. In the course of the following 72 pages the constancy of the angles of crystals, symmetry, notation, drawing of crystal forms and spherical projection, are in turn explained. The physical properties of crystals are then briefly touched upon, and in a last lesson mero-symmetry is discussed. The appendix (55 pages) deals with a subject entirely different from Crystallography, namely Determinative Mineralogy, and is made up almost wholly of tables drawn up after the manner of those of Dr. Weisbach. The book contains numerous woodcuts in the text, and is furnished with four plates, two of them consisting of diagrams to be pasted on cardboard and used in the construction of the aforementioned models. To each lesson is appended a set of useful questions relating to the subject which has been discussed. Some of the statements are wanting in accuracy: for instance, on page 55 the student is told that "the symbols of all planes in a zone have two of their indices always in a constant ratio," which is untrue; and at times the language is wanting in neatness and precision: still, if the student is in the hands of a careful teacher, he will be able to get much help from the book, and is not likely to be led astray.

By the Deep Sea; a Popular Introduction to the Wild Life of the British Shores. By Edward Step. Pp. 322. (London: Jarrold, 1896.)

THE author of this little volume is already favourably known by his popular books on wild flowers, &c., and the present work will add to his reputation as a writer for the non-scientific reader. The author's endeavour has been to introduce to the seaside visitor a large number of the interesting creatures to be found on the rocks, the sands and the shingle, and he claims to have written the whole of the work in close contact with the objects he describes—not only of cabinet specimens, but of the living creatures under natural conditions. In his own words: "There is not a line in the whole volume that has not been written within a few yards of, and in full view of the rocks." The twenty chapters into which the book is divided are devoted to the sea and its shores, low forms of life, sponges, zoophytes, jelly-fishes, sea-anemones, sea-stars and sea-urchins, sea-worms, crabs and lobsters, shrimps and prawns, some minor crustaceans, barnacles and acorn-shells, "shell-fish," sea-snails and sea-slugs, cuttles, sea-squirts, shore fishes, birds of the seashore, sea-weeds, flowers of the shore and cliffs. The style of writing is easy and attractive, and the text is further elucidated by the insertion of a number of well-chosen, if somewhat rough, illustrations from the works of P. H. Gosse, and others which appear to have been specially drawn for the work. Many a seaside holiday will be more fully and permanently enjoyed by the study of this tastefully got-up little book, the usefulness of which is increased by a general and a classified index.

LETTERS TO THE EDITOR.

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The Utility of Specific Characters.

I HOPED that I might have held my peace on this subject. Prof. Lankester, however, complains, and not for the first time, that I have misrepresented, or at any rate misunderstood him.

I do not doubt his acquaintance with Prof. Weldon's work, though he has allowed a long time to elapse before criticising it. I am glad that he regards it as "interesting and valuable." But this is what he said about it in NATURE for July 16 last:—

"Such methods of attempting to penetrate the obscurity which veils the interactions of the immensely complex bundle of phenomena which we call a crab and its environment, appear to me not merely inadequate, but in so far as they involve perversion of the meaning of accepted terms and a deliberate rejection of the method of inquiry by hypothesis and verification, injurious to the progress of knowledge."

It is quite true that Prof. Lankester has not said in so many words that "Prof. Weldon's investigation of the crab's carapace 'does not satisfy the canons of scientific inquiry.'" But it appears to me that this is a very mild way of putting what he did say.

I expressed the opinion that Prof. Weldon's investigation did rest on an hypothesis, and that this was subjected to verification. Whether the hypothesis was reasonable and the verification adequate is a matter on which Prof. Karl Pearson and others are entitled to form their own judgment.

Kew, September 28. W. T. THISELTON-DYER.

I FEEL grateful to Prof. Karl Pearson for his lucid and rational contribution to this discussion, in which it has sometimes seemed to me that the main question was in danger of being obscured by more or less irrelevant arguments.

I pointed out in a letter to NATURE, soon after the publication of Prof. Weldon's report last year, that he had not, and had not claimed to have, proved that there was a differential or selective death-rate in shore crabs, with respect to variations of their frontal breadth. He showed that the curve of variation in larger (and therefore presumably older) crabs was different from that in smaller crabs. The departures from the mean were less. He concluded, that if this difference were not due to growth-changes it must be due to the death of crabs with extreme variations. But on the other hand it had to be proved that the difference was not due to growth-changes. Changes in the proportions of parts are so common during growth in so many animals, that it seemed to me much more likely that the difference discovered by Prof. Weldon was due to such changes than to a differential death-rate. I understand that he has since been investigating what he calls the law of growth in these crabs, but so far as I know he has not published any further results.

I am glad to find that Prof. Karl Pearson's opinion concerning the conclusions to be drawn from the evidence published by Prof. Weldon, entirely agrees with mine. It would be very interesting to learn now whether Prof. Weldon is able to settle the question of the changes occurring in the growth of shore crabs, and either to confirm or withdraw his suggested conclusion that the difference he described was due to selective death-rate. It would take a good deal of evidence to convince me that shore crabs in which the frontal breadth differed slightly from the mean, died in greater numbers than those in which it was nearer the mean. But if the evidence is forthcoming, I am ready to accept it. It seems to me that Mr. Thiselton-Dyer is inclined to accept the conclusion before the evidence is forthcoming. He seems to have overlooked the other possible explanation of the result, namely changes in the same crabs during growth.

I also maintained in my letter last year, as Profs. Lankester and Karl Pearson maintain now, that if a differential death-rate were demonstrated, it would still be necessary to discover how that death-rate was caused, what was the relation between the character in question and the conditions of life which caused individuals with certain variations of the character to die off.

I do not profess to be a specialist in logic, but it seems to me that the fallacy into which Prof. Weldon has fallen is that of confounding the categories. He maintains that if a certain

variation is correlated with a certain death-rate, it must be the cause of it, and that it is not possible to distinguish between variations which are directly useful, and those which are only physiologically correlated with the useful. But it seems to me that this is like talking of hitting a nominative case with a stick. The variation is a magnitude in an organism, survival or death is a relation between the organism and its environment. It is the relation of the variation to life which alone can be said to be the cause of death or survival. The relation to the conditions of life is advantage, disadvantage, or neutrality in the struggle for existence. If I have stated the logic of the matter correctly, I venture to think that the apprehension of this principle is a necessary preliminary to any attempt to demonstrate empirically the occurrence of natural selection.

Prof. Weldon's chief contention was that by the statistical method, when the law of growth of the characters examined was known, a measure of the rate and direction of the evolution of an organism could be obtained. Such a measure would be afforded by the selective death-rate. But he has not yet demonstrated a selective death-rate in a single instance. And further, a measure of the rate and direction of evolution has nothing to do with the cause of the selective death-rate. If characters of no apparent utility are proved to be subject to selection, there still remains the question how the selection is brought about. Measures of the rate and direction of the wind do not tell us the cause of the wind. They may help us to discover the cause, and I have no doubt that Prof. Weldon's investigations are a valuable contribution to the investigation of evolution. But it is only when it has been shown that the degree of utility of a variation, or its correlation with useful variations determines its survival, that the occurrence of natural selection has been demonstrated. J. T. CUNNINGHAM.

September 19.

Fossil Tridacnids in the Solomon Islands.

SOME months ago, on the voyage between New Guinea and Sydney, the small trading steamer on which I travelled called at a number of islands in the British Solomons, the first station at which we called being Rubiana, in the little-known island-complex of New Georgia. Here I became acquainted with the heavy arm-rings worn by the natives, and obviously made from the shell of *Tridacna* or *Hippopus*. What was very surprising, however, was the information which I obtained from all quarters and from different localities, from blacks as well as from whites, that these arm-rings are not made from recent shells found on the reef, but from shells obtained far away in the interior, or, as they say, in the bush. At first sight, the arm-rings, above referred to, strongly remind one of those made from the recent *Tridacna* by the natives of the Sir Charles Hardy Island, which lies to the north of the Solomon Group; but while the former are solid rings more than half an inch in thickness, the latter are deeply grooved on the outer border.

This difference is shown in Figs. 1 and 2, which represent cross-sections through the arm-rings of the Solomon and Sir

FIG. 1.

FIG. 2.

Charles Hardy islanders, respectively. But there are other differences, not so much of artistic as of economic importance. The grooved rings are much more readily obtained from the natives who make and wear them, than are the solid rings. The latter have a great value among the natives themselves, and when they are shot with a vein of reddish or reddish-yellow colour (derived no doubt from the hinge-line, which also gives their beauty to the nose-pieces of the New Guinea natives), they can only be mentioned with bated breath.

The reason why the Solomon Islanders prefer the ancient to the recent shells, lies possibly in the fact that, as a general rule, among the natives of the larger islands of the Pacific, the artists and artificers (apart from the making of canoes) are to be found among the bush-natives, rather than among those who live in proximity to the sea. The latter are traders, *par excellence*—men of the world who do their business in great waters. The former live in primitive innocence, are possessed of uncouth manners, and produce poets, magicians, medical men, and professional dancers, together with workers in wood and stone. To the last-mentioned members of the community, therefore, the Tridacnid shells, when they occur in the bush through elevation of a former coral reef, are ready conveniently to hand.

I have thought it worth while to draw the attention of naturalists to the above indication of the existence of upraised coral reefs in the Solomon Islands, which would be well worth an attentive examination, and, while in Sydney, Mr. R. Etheridge, jun., informed me that he knew of other instances in the Pacific of coral reefs having been raised to an elevation of over a thousand feet. ARTHUR WILLEY.

Nouméa, New Caledonia, July 16.

Visual Aid in the Oral Teaching of Deaf Mutes.

PROBABLY every one is acquainted with Kœnig's manometric capsules and revolving mirrors, and it occurred to me that I might help a deaf mute to learn inflection in speaking by his imitating the curves produced by my voice in the mirrors. For this purpose I arranged two capsules with oblique membranes and small diameter side by side, one being higher than the other, so that two bands of flame half inch wide, and half inch apart, appeared in the revolving mirrors. The capsules were tuned alike, and furnished with tubes and conical mouthpieces; through one of these I made the sound of a note, vowel, or syllable in various pitches, and my friend endeavoured to imitate through the other tube the curve in the flame band produced by my voice. As an experiment the results were quite satisfactory, for before an hour was over he could imitate a range of nearly an octave, and would tell me correctly, through watching the curves of flame, when the note he uttered was like mine. I am not interested in the oral teaching of the deaf, but having frequently to use Kœnig's invention, I think the principle might be made useful to oral teachers. My friend, upon whom I experimented, is said to have been well taught, his age about twenty years, but his voice (?) is a hoarse monotone. T. HAWKSLEY.

11 Primrose Hill Road, N.W.

INTERNATIONAL METEOROLOGICAL CONFERENCE AT PARIS.

THIS Conference was held at the rooms of the Société d'Encouragement, in the Rue de Rennes, from September 17 to 23. About forty members were present. M. Mascart was elected President, MM. de Bezold and Tacchini Vice-Presidents, and MM. Angot, Erk, and Scott Secretaries. The complete report of proceedings has not yet been printed. It was decided that Committees should be appointed to continue the investigation of several subjects, viz.:

I. Terrestrial Magnetism and Atmospheric Electricity; Prof. Rücker (President).

II. Clouds; Prof. Hildebrandsson (President).

III. Radiation and Insolation; M. Violle (President).

IV. Aerostatics and Balloon Work; Prof. Hergesell (President).

On the motion of Mr. Symons, the International Meteorological Committee was reappointed with a few changes, rendered necessary by the respective resignations of Prof. Wild, Prof. Harrington, and Mr. Ellery. The President is Prof. Mascart, and the Secretary Mr. R. H. Scott.

ARMAND HIPPOLYTE LOUIS FIZEAU.

BY the death of M. Fizeau physical science has lost one who will rank high among those who have contributed to the scientific distinction of the nineteenth century. Every student of optics knows M. Fizeau's beautiful experimental method of determining the velocity of light; but not so many are aware of the other re-

markable researches by which he has partially answered some of the most difficult questions as to the relation of matter to ether, which are perplexing the best physical investigators of the time.

Born in 1819, Fizeau was only thirty years of age when his paper, "Sur une expérience relative à la vitesse de propagation de la lumière," appeared in the *Comptes rendus*. In this he put forward his plan of rotating a wheel having round its rim alternate teeth and spaces of equal width, so that these teeth and spaces should alternately intercept and allow to pass a beam of light from a source, and so adjusting the speed of rotation that the time occupied by the light in travelling from the wheel to a mirror and back again, should be equal to the time taken by the rim of the wheel to advance through a space equal to an integral number of times the width of a tooth or space. Curiously enough, the other experimental method of finding the velocity of light was described by Foucault in the very next volume of the *Comptes rendus*. In some respects the latter method —that of the revolving mirror—was even more striking than that of Fizeau. It allowed the velocity of light to be determined within an ordinary room, and, besides, enabled the question as to whether light travelled more or less quickly through a more refractive medium to be decided by direct experiment.

Another experiment of capital importance with which the name of Fizeau will ever be honourably associated is that by which he determined the amount of drift of light-waves in a transparent medium in motion. According to a theory given by Fresnel, the velocity of drift of ether-waves in a medium moving with velocity u is $(1-1/\mu^2)u$, where μ is the index of refraction of the medium. This conclusion of Fresnel was verified more lately by the experiments of Airy and Hoek, which proved, in opposition to the statement of Klinkerfues, that no change in the constant of aberration is observed when the tube of the observing telescope is filled with water. But it was tested directly by Fizeau in the most simple and beautiful manner. Two tubes were arranged side by side, and water was forced at a considerable speed (as much as seven metres per second) along one tube and back by the other, while a beam of light was split into two parts, which were sent round the tubes, one with the stream, the other against the stream, and then brought together again and tested for interference produced by the virtual difference of path traversed, arising from the motion of the water. The result gave exactly the formula quoted above, and has been confirmed by very careful experiments made comparatively recently by Michelson and Morley.

Fizeau made some notable observations on the number of interference bands observable with approximately homogeneous light, and, in conjunction with Foucault, carried out a most important series of observations on the light in different parts of the field of illumination in interference experiments. The method consisted in applying the spectroscope to examine the light taken from a narrow part of the field parallel to the bands, and proved *inter alia* that there is really interference in that region of the field which seems to be uniformly illuminated in consequence of overlapping produced by want of perfect homogeneity of the light.

One very important recent result of such observations has been to show that the detection of interference is limited only by the resolving power of the spectroscope employed, and that the usual inference as to the regularity of the vibrations in a source of light is unjustifiable.

Like Joule in this country, Fizeau carried on scientific research largely from his own private resources; and by a long series of most valuable papers published in the Memoirs of the French Academy and elsewhere, he has earned the gratitude of his countrymen and the world. But his most enduring memorial will doubtless be his determination by simple laboratory apparatus of the velocity of light (a velocity sufficient to enable the earth's path round the sun to be traversed in about twenty-six minutes!), and with his great colleague Foucault he will be held in honoured remembrance so long as men study the science of optics.

Fizeau was elected a Foreign Member of the Royal Society in 1875, and he received the Rumford Medal of the Society in recognition of his scientific work.

A. GRAY.

NOTES.

THE monument to Lobachevsky, erected at Kazan, in a square which bears the name of the great geometer, was unveiled on September 13, in the presence of the Bishop of Kazan, the Governor of the province, the University, the local Physical and Mathematical Society, and a great number of sympathisers. The Mayor of Kazan made a statement as to the funds raised for the erection of the monument. Prof. Suvoroff referred to the scientific work of Lobachevsky in mathematics and physics, and Prof. Vasilieff spoke of the great geometer as one whose life was worthy of emulation, and as an energetic worker for spreading scientific knowledge. In the evening the Physical and Mathematical Society held a special commemoration meeting before a distinguished gathering of visitors of both sexes.

A SERIES of *fêtes* have been celebrated at Alais, in the centre of the great mulberry and silkworm district of France, in commemoration of the services rendered by Pasteur to sericulture. A statue of Pasteur was unveiled during the celebrations; and, on Saturday last, a solemn service was celebrated in the cathedral in commemoration of the first anniversary of his death, which occurred on September 28, 1895.

THE Harveian oration is to be delivered before the Royal College of Physicians, on October 19, by Dr. J. Frank Payne.

IT is proposed to establish an International Botanical Station at Palermo, under the superintendence of Prof. Borzi, who desires the co-operation of botanists of all countries.

DR. A. ZIMMERMANN has been appointed botanist to the section of the Botanic Garden, Buitenzorg, Java, devoted to the cultivation of coffee.

THE Graefe gold medal, which is awarded by the German Ophthalmological Society every ten years, has this year been awarded to Prof. Theodore Leber, of Heidelberg, in recognition of his work on inflammation. Prof. von Helmholtz was the first to receive the medal, the award being made for his discovery of the ophthalmoscope, and his treatise on physiological optics.

A GAS exposition, beginning on January 25, 1897, is to be held for two weeks in the Madison Square Garden, New York. The object of the exposition, according to the prospectus, is to bring together a collection of gas apparatus and appliances of every description, for the purpose of affording the general public and the gas engineer an opportunity to study the developments that have taken place in the gas industry during recent years.

THE French Medical Press Association is organising a memorial festival in honour of the jubilee of the discovery of anæsthesia. The festival will take place in Paris, on October 18 and following days. The programme includes a ceremonial meeting at the Sorbonne, a banquet, and a special performance at one of the theatres. A suitable commemoration of the event is being arranged in Boston (Mass.), where the first

surgical operation under ether was performed on October 16, 1846. The Society of Anæsthetists, of London, is also taking steps to celebrate the occasion in a fitting manner.

THE Peary expedition has returned to Sydney, Cape Breton, from Greenland, but without the great meteorite which it hoped to bring back. This enormous block of metal, which Lieut. Peary set out to fetch, weighs about forty tons, and is situated on an island twenty miles inside Cape York. The jackscrews designed to lift the mass upon shipboard proved not to be strong enough, so another journey will have to be undertaken to secure it. Meanwhile we trust that the Esquimaux, who have used the meteorite as a source of workable iron for many years, will not greatly reduce the mass before another attempt is made to remove it. In spite of this disappointment, the members of the expedition have not returned empty-handed; for their collections and observations appear to be valuable and varied.

THE meeting of the American Public Health Association was held at Buffalo, September 15–18, the President, Dr. Eduardo Licéaga, of Mexico, in the chair. Delegates, thirty-five in all, were present from nearly every State in Mexico, from most of the United States, and from Canada. The work of sanitation thus received an impetus throughout the American continent; and such diseases as yellow fever, small-pox, diphtheria, &c., are waning under the vigilant efforts of the combined army of health officers. Diligent attention to business, and rigid enforcement of time limits, enabled the Association to complete a long and valuable programme of reports and papers, besides adopting several important resolutions. As to the place of meeting next year, the advisory council recommended Toronto. Motions were made to substitute Nashville and Philadelphia, and the final vote favoured the latter, which will accordingly be the place of the next (being the twenty-fifth) meeting of the Association. The officers elected are:—President, Dr. H. B. Hornbeck, of Charleston, S.C.; first Vice-President, Dr. Peter H. Bryce, of Toronto, Canada; second Vice-President, Dr. Ernest Wende, of Buffalo, N.Y.; Treasurer, Dr. Henry H. Holton, of Brattleboro', Vt.

WE regret to record the death of Sir John Erichsen, the distinguished surgeon, at the age of seventy-eight. He was elected a Fellow of the Royal Society in 1876, and since 1887 had been President of University College, London.

SIR GEORGE M. HUMPHRY, F.R.S., Professor of Surgery in the University of Cambridge, died on Thursday last, and by his death the University is deprived of one through whose exertions the medical school has been brought to the present high position. He became Professor of Anatomy in the University in 1866, and Professor of Surgery in 1883. His life affords an instance of the manner in which the development of a subject is dependent upon the bearing of University authorities towards it. When he was appointed to the chair in the University, he set to work, in conjunction with the late Sir George Paget, with the object of placing the study of medicine and surgery in a more prominent and satisfactory position. To quote the *Times*: "It was a task of great difficulty, for, although as far back as 1851 the Natural-Science Tripos had been in existence, yet it attracted but a handful of students for the first twenty years. The first real impetus given to the work was when some of the colleges recognised the Natural-Science Tripos as one avenue to a fellowship. The standard of the examinations, both for the Tripos and for medical degrees, was raised, examiners not connected with the University were appointed, and open scholarships for Natural Science were offered. The result is that at the present time the Natural-Science Tripos attracts more students than any other of the honour examinations in the University, the medical school is one of the largest in the country, and the medical degrees of the University are held in the highest estimation by the profession. He has left behind him an array of excellent professors and teachers, and has placed the study of medicine and surgery in the front rank at the University. His exertions entitle him to be regarded as one of the greatest benefactors to the University in modern times." His chief scientific work was in comparative anatomy, to which branch of knowledge he made some important contributions. Among his best-known works are "A Treatise on the Human Skeleton," 1858; "On Myology," 1872; "Old Age, and Changes incidental to it," 1889.

WITH reference to the recent disastrous gales and torrential rainfall over the British Islands, the *Weekly Weather Report* of September 26 shows that the changes brought about in barometric pressure were very great, amounting to considerably over an inch in twenty-four hours at many places. The rainfall was much in excess in all districts, especially in the western parts of the country; falls exceeding an inch in twenty-four hours have been reported on several days. The total rainfall since the beginning of the year is still below the average, except in the north of Scotland and the north of Ireland. The greatest deficiency is in the south-west of England, where it amounts to nearly seven inches.

A LUNAR bow, in which the various prismatic colours could be distinguished without difficulty, was observed at Portmadoc, North Wales, last Sunday evening, by Mr. Walter Williams, who has sent us a description of the phenomenon. The time at which the bow was seen was 9.40 p.m. The colours appeared on the western edge of a dark rain-cloud, which was moving rapidly towards the east. This cloud was apparently very much in advance of another thin cloud, of pearly whiteness, surrounding the moon's disc; nevertheless the two clouds seemed continuous, and the soft silky whiteness of the one formed a sharp contrast to the coloured bow on the edge of the other. There were no more clouds in the immediate vicinity of the moon. The bow was visible for a length about twelve times greater than the moon's apparent diameter. Violet was the innermost colour, and there was a sharp contrast between it and the white cloudiness. The whole phenomenon only lasted four minutes.

IN a valuable memoir recently published in the *Annali* of the Central Meteorological Office of Rome, Prof. Arcidiacono describes the Syracuse earthquake of April 13, 1895, which disturbed the whole of the south-east corner of Sicily. The centre of the epicentral area is at the village of Vizzini, and its longer axis lies along a line joining this place with Cape Passero. It is interesting to notice that this line coincides nearly with the axis of the ridge of Monte Lauro, and also joins the two principal volcanic centres (now extinct) of the Val di Noto. At the epicentre the intensity was 9, according to the Rossi-Forel scale; in other words, the shock was sufficiently strong to damage buildings, but not to destroy them entirely. On the map which illustrates his paper, Prof. Arcidiacono shows the course of eight isoseismal lines, and, using the method of Dutton and Hayden, estimates the depth of the seismic focus to be about 7·4 km.

IN the *Revue Scientifique* (No. 11) will be found the communication made by M. Stokvis to the International Colonial Institute on the question of colonisation in tropical regions. The author speaks strongly in favour of successful colonisation by Europeans in low latitudes, and brings evidence together showing that with due regard to hygiene the European is practically as well off as the native. The conclusions which he eventually arrives at are: (1) That the establishment and prosperity of European colonies, whether they be for purposes of "exploitation" or agriculture, are perfectly possible in both

high and low tropical regions. (2) In the question of colonisation, tropical temperatures and the race of the colonist play only secondary *rôles*. (3) Colonisation on a large scale—that is colonisation of the masses—ought to be stopped.

A NEW volume of the late Baron Uslar's great work on the languages of the Caucasian mountaineers has just been issued at Tiflis, by the Department of Education. It is devoted to the Kyurin language. The Kyurins are a small stem, inhabiting the banks of the Samur river, in the north of the Daghestan plateau. For a long time they were under the rule of the khans of Derbent, Kuba, or Kazikumukh; but in the second half of the last century a separate Kyurin khanate came into existence, and maintained itself up to 1866, when it was conquered by the Russians. The Kyurin language, which has many sub-branches, must be considered as an independent linguistic unit, while its pronunciation varies with nearly every separate village. The first part of Baron Uslar's work contains a description of the leading features of the language and its grammar; while the second part is a dictionary of Kyurin words.

The current number of the *Zoologist* contains an interesting article, by Mr. A. Holte Macpherson, on "Some Observations on the Note of the Cuckoo." During the spring and early summer of the present year the author took every available opportunity of listening to the cuckoo, and enlisted in his service many friends to do the same, his purpose being to determine, if possible, the pitch of the bird's voice, and the duration of the interval between the notes of its call. An analysis of the reports in his hands shows that when the bird is in full song the interval is usually greater than the minor third, and is to all intents and purposes a full major third. Not infrequently the bird utters three notes. At Haileybury, on June 7, it was heard to sing E flat, D C two or three times, then it omitted the middle note, singing a minor third. Two other birds are reported to have sung F F C and F, D flat, and C, respectively. As regards the pitch, out of hundreds of recorded calls during the period when the bird was in good voice, the upper note in nineteen cases out of twenty was from F to E flat, and the lower note from D to B. The author comes to the conclusion that the average call is E and C in the middle of the piano.

WITH its current issue, our contemporary, *Science Progress*, enters upon a new phase of its existence. It has been enlarged, and will in future appear quarterly, instead of monthly, at a slightly increased price.

Bulletin No. 57 of the Experiment Station of the Kansas State Agricultural College, Manhattan, is occupied by a descriptive list of "Kansas Weeds," accompanied by upwards of twenty plates of drawings of the leaves or other characteristic organs.

Timehri (June), the journal of the Royal Agricultural and Commercial Society of British Guiana, contains the following scientific contributions: "Multiple Evaporation," by W. P. Abell; "Queer Homes" (an account of nests built in peculiar places), by C. A. Lloyd; "Note on Berbice Bats," by Dr. C. G. Young; "India-Rubber Collection at Para," by J. A. Coelho. In addition to the foregoing, there are a number of articles of commercial interest.

MR. BERNARD QUARITCH has sent us his catalogue, dated September, containing a great many works relating to mathematics, microscopy, mountaineering, naval sciences, ornithology, palæontology, travels and zoology. Among the books mentioned we notice a complete set of the "Philosophical Transactions of the Royal Society," with general indexes; a set of the "Transactions of the Linnean Society," from 1791 to 1891; the "Proceedings and Journal of the Linnean Society," from 1838 to 1895; and the "Proceedings of the Zoological Society," from 1830 to 1894.

A CATALOGUE of meteorites in the collection of the American Museum of Natural History, New York, by Mr. E. O. Hovey, has been received. The collection consists of fifty-five slabs fragments and complete objects, representing twenty-six falls and finds. The source of each specimen, also the dates of discovery, and the individual weights in grams, is given in the catalogue, which should be of interest and service to many visitors to the museums and others.

WE have received the *Bulletin of Miscellaneous Information* of the Royal Botanic Gardens, Trinidad, for July. Among the "Natural History Notes" is a very interesting account of the life-history of the parasol ants, *Atta cephalotes* and *octospinosa*, with drawings of the various forms—the male, queen, soldier, worker major, worker minor, nurse, and gardener. Mr. J. H. Hart, the Superintendent, confirms the statement of Belt that these ants carry vegetable matter into their nests, not as food, but as a material on which to grow the fungi on which they feed. The destruction caused by various species of parasol ant in the Western Tropics is a matter of very serious importance to the agricultural industries.

WE have received the *Bulletin Meteorologique et Seismique de l'Observatoire Imperial de Constantinople* for February of this year. In this is given a list of the earthquakes observed during this month in the East, and more especially those occurring in the Ottoman Empire. The number seems to be considerable, no less than twenty-nine being described. The meteorological observations for this month are also given, the Director of the Observatory, Salib Feky, adding a *résumé* and his usual monthly *revue climatologique*.

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus sinicus*, ♀ ♀) from India, presented by Mrs. Strutt; a Macaque Monkey (*Macacus cynomolgus*, ♀) from India, presented by Mr. J. Laverock; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Miss M. E. Clarke; a —— Squirrel (*Sciurus* sp. ?) from Monravia, West Africa, presented by Mr. Ellis Edwards; an Orange-cheeked Amazon (*Chrysotis autumnalis*) from Honduras, presented by Mr. Baratti; a Common Heron (*Ardea cinerea*), British, presented by Mr. E. J. Poyser; four Montague's Harriers (*Circus cinereaceus*), British, presented by Mr. W. J. Laidlay; three Pin-tailed Sand Grouse (*Pterocles alchata*) from Spain, presented by Mr. G. P. Torrens; six Rough Terrapins (*Clemmys punctularia*) from Para, presented by Dr. E. A. Goeldi; a short-tailed Wallaby (*Halmaturus brachyurus*) from Australia, deposited; two Ruffs (*Machetes pugnax*), British, purchased; an African Lepidosiren (*Lepodosiren annectans*) from West Africa, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE SOLAR ROTATION.—In the August number of the *Astrophysical Journal* there is a brief summary of the work being done at John Hopkins University by Mr. Lewis Jewell. These researches dealt with the question of the solar rotation, and Mr. Jewell's recent work in measuring a large number of lines in photographs of the solar spectrum has brought out, as is stated, a new and remarkable peculiarity in the law of the solar rotation. The following is a brief extract of the note in question. "It is found that there is a difference of several days in the rotation periods of the outer and inner portions of the sun's atmosphere, the period increasing as the photosphere is approached. The measures also show the equatorial acceleration to be much the greatest for the outer portions of the atmosphere. At the lower levels the acceleration is small, there being little difference in the periods for different latitudes. It is further found that the carbon (cyanogen) lines and the shaded portions of H and K take their rise very low down in the solar atmosphere. Mr. Jewell is at present engaged upon the reduction of the measures."

A NEW SPECTROSCOPIC BINARY.—Prof. E. C. Pickering, in Circular (No. 11) of the Harvard College Observatory, dated August 31, informs us that Prof. Solon I. Bailey has found μ^1 Scorpii to be a spectroscopic binary. This star is $-37^\circ\cdot11033 = S.M.P.\ 5794$; its approximate position for 1900 is R.A. 16h. 45·1m. Decl. $-37^\circ\ 53'$, its photometric magnitude being 3·26. A neighbouring star μ^2 Scorpii follows about 28s., is 1·7 north, with a photometric magnitude of 3·74. As these two stars were close alongside on the photographic plate, a comparison was easy. The spectrum o. the first-named is described as of the first type, with the additional lines characteristic of the Orion stars. In some of the spectra they are scarcely distinguishable, while in others the lines of the first are broad and hazy, some, more faint, being distinctly double. Mrs. Fleming, who examined these plates in 1894, recorded these lines as being double, but the plates were put away for further examination, and subsequently overlooked. An examination of the three plates sent to Cambridge showed that the lines in the spectrum of μ^1 were single on October 2, 1892, wide and hazy on July 20, 1894, and double on July 31, 1894. A more minute examination has shown that the changes are very rapid, a period of 35 hours and a nearly circular orbit having been deduced by Prof. Bailey from a discussion of fifty-two photographs. An independent discussion at Harvard gives the average period of 34h. 42·5m., with an error of less than 6s. Ten observed times, when the lines were single, are represented with an average deviation of 38 minutes each; the maximum deviation is less than an hour. Other stars of this class, only two of which are already known, are ζ Ursæ Majoris and β Aurigæ. The former was discovered by Prof. Pickering in 1889; it has a period of 52 days, and is irregular. The latter we owe to Miss A. C. Maury; the period of this is regular, and is of nearly four days in length.

THE VARIABLE STAR Z HERCULIS.—A point of great importance, but not sufficiently attended to by those who compute variable star observations, is referred to by Mr. Paul S. Yendell in *Astronomical Journal*, No. 20. It is well known that outstanding observations—that is, those which seem apparently to be incorrect—are generally discarded, as leading to erroneous results in the final reduction. This is often done, for instance, when a curve is drawn through the points, representing the observations, and finally smoothed to include, as near as possible, all the data. This smoothing is carried, in some cases, to a considerable extent; in fact so far that a slight hump in the curve is looked upon as evidently due to errors of observation, and consequently smoothed over, and therefore lost so far as the results are concerned. Mr. Yendell refers to a similar "smoothing" by the rejection of observations which do not bear out the hypothesis of the calculated orbit. In the note in question, he takes the case of the four observations, made by Müller and Kempf, of the variable star Z Herculis, for the Potsdam Photometric Durchmusterung. The first and last observations satisfy the elements of Hartwig, but these latter are not in accord with observations made by Yendell in 1895. Duner's elements, on the other hand, are found to satisfy the observations of 1894 and 1895, but not those made at an earlier date at Potsdam. Mr. Yendell thus concludes that the star's period must evidently be variable, though, as he says, the character and value of the variation cannot at present be determined. He objects, however, strongly to Prof. Duner's allusion to one of his (Yendell's) observations as "evidently erroneous." This observation, as Yendell remarks, "happens to be one of the best defined and best observed of the entire series, and entirely free from any suspicion of prepossession, as is indicated by the weight attached to it."

The value of an apparently outstanding observation is further instanced by Yendell in the case of the star U Pegasi, observed by him in 1894, which he had been inclined to pass over lightly as "hopelessly discordant," but which proved in reality to have "contained the key to the whole enigma of the star's period." Other instances might be given of similar cases; but sufficient has been said to draw attention to the fact that the light of variable stars is of a more variable nature than is at present supposed. Recent observations and reductions have shown that the curves representing variability of some stars is not a simple rise to maximum and fall to minimum, but the curvature varies both on the upward and downward side of the light curve to no slight extent. Cases of this kind seem to point to the suggestion that more than two bodies are involved.

THE BRITISH ASSOCIATION.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY ARTHUR J. EVANS, PRESIDENT OF THE SECTION.

"The Eastern Question" in Anthropology.

TRAVELLERS have ceased to seek for the "Terrestrial Paradise," but, in a broader sense, the area in which lay the cradle of civilised mankind is becoming generally recognised. The plateaus of Central Asia have receded from our view. Anthropological researches may be said to have established the fact that the white race, in the widest acceptation of the term, including, that is, the darker-complexioned section of the South and West, is the true product of the region in which the earliest historic records find it concentrated. Its "Area of Characterisation" is conterminous, in fact, with certain vast physical barriers due to the distribution of sea and land in the latest geological period. The continent in which it rose, shut in between the Atlantic and the Indian Oceans, between the Libyan Desert, and what is now Sahara, and an icier Baltic stretching its vast arms to the Ponto-Caspian basin, embraced, together with a part of anterior Asia, the greater part of Europe, and the whole of Northern Africa. The Mediterranean itself—divided into smaller separate basins, with land bridges at the Straits of Gibraltar, and from Sicily and Malta to Tunis—did not seriously break the continuity of the whole. The English Channel, as we know, did not exist, and the old sea-coast of what are now the British Islands, stretching far to the west, is, as Prof. Boyd Dawkins has shown, approximately represented by the hundred-fathom line. To this great continent Dr. Brinton, who has so ably illustrated the predominant part played by it in isolating the white from the African black and the yellow races of mankind, has proposed to give the useful and appropriate name of "Eurafrica." In "Eurafrica," in its widest sense, we find the birthplace of the highest civilisations that the world has yet produced, and the mother country of its dominant peoples.

It is true that later geological changes have made this continental division no longer applicable. The vast land area has been opened to the east, as if to invite the Mongolian nomads of the Steppes and Tundras to mingle with the European population; the Mediterranean bridges, on the other hand, have been swept away. Asia has advanced, Africa has receded. Yet the old underlying connection of the peoples to the north and south of the Mediterranean basin seems never to have been entirely broken. Their inter-relations affect many of the most interesting phenomena of archæology and ancient history, and the old geographical unity of "Eurafrica" was throughout a great extent of its area revived in the great political system which still forms the basis of civilised society, the Roman Empire. The Mediterranean was a Roman lake. A single fact brings home to us the extent to which the earlier continuity of Europe and North Africa asserted itself in the imperial economy. At one time, what is now Morocco and what is now Northumberland, with all that lay between them on both sides of the Pyrenees, found their administrative centre on the Mosel.

It is not for me to dwell on the many important questions affecting the physiological sides of ethnography that are bound up with these old geographical relations. I will, however, at least call attention to the interesting, and in many ways original, theory put forward by Prof. Sergi in his recent work on the "Mediterranean Race."

Prof. Sergi is not content with the ordinary use of the term "White Race." He distinguishes a distinct "brown" or "brunette" branch, whose swarthier complexion, however, and dark hair bear no negroid affinities, and are not due to any intermixture on that side. This race, with dolichocephalic skulls, amongst which certain defined types constantly repeat themselves, he traces throughout the Mediterranean basin, from Egypt, Syria, and Asia Minor, through a large part of Southern Europe, including Greece, Italy, and the Iberic peninsula, to the British islands. It is distributed along the whole of North Africa, and, according to the theory propounded, finds its original centre of diffusion somewhere in the parts of Somaliland.

It may be said at once that this grouping together into a consistent system of ethnic factors spread over this vast yet inter-related area—the heart of "Eurafrica"—presents many

attractive aspects. The ancient Greek might not have accepted kinship even with "the blameless Ethiopian," but those of us who may happen to combine a British origin with a Mediterranean complexion may derive a certain ancestral pride from remote consanguinity with Pharaoh. They may even be willing to admit that "the Ethiopian" in the course of his migrations has done much to "change his skin."

In part, at least, the new theory is little more than a re-statement of an ethnographic grouping that commands a general consensus of opinion. From Thurnam's time onwards we have been accustomed to regard the dolichocephalic type found in the early Long Barrows, and what seem to have been the later survivals of the same stock in our islands, as fitting on to the Iberian element in South-western Europe. The extensive new materials accumulated by Dr. Garson have only served to corroborate these views, while further researches have shown that the characteristic features of the skeletons found in the Ligurian caves, at Cro Magnon and elsewhere in France, are common to those of a large part of Italy, Sicily, and Sardinia, and extend not only to the Iberic group, but to the Guanche interments of the Canary Islands.

The newly correlated data unquestionably extend the field of comparison; but the theories as to the original home of this "Mediterranean Race" and the course of its diffusion may be thought to be still somewhat lacking in documentary evidence. They remind us rather too closely of the old "Aryan" hypothesis, in which we were almost instructed as to the halting places of the different detachments as they passed on their way from their Central Asian cradle to rearrange themselves with military precision, and exactly in the order of their relationship, in their distant European homes. The existing geological conditions are made the basis of this migratory expansion from Ethiopia to Ireland; parallel streams move through North Africa and from Anatolia to Southern Europe. One cardinal fact has certainly not received attention, and that is, that the existing evidence of this Mediterranean type dates much further back on European soil than even in ancient Egypt.

Prof. Sergi himself has recognised the extraordinary continuity of the cranial type of the Ligurian caves among the modern population of that coast.

But this continuity involves an extreme antiquity for the settlement of the "Mediterranean Race" in North-western Italy and Southern France. The cave interments, such as those of the Finalese, carry back the type well into Neolithic times. But the skeletons of the Baoussé Roussé caves, between Mentone and Ventimiglia, which reproduce the same characteristic forms, take us back far behind any stage of culture to which the name of Neolithic can be properly applied.

The importance of this series of interments is so unique, and the fulness of the evidence so far surpasses any other records immediately associated with the earliest remains of man, that even in this brief survey they seem to demand more than a passing notice.

So much, at least, must be admitted on all hands: an earlier stage of culture is exhibited in these deposits than that which has hitherto been regarded as the minimum equipment of the men of the later Stone Age. The complete absence of pottery, of polished implements, of domesticated animals—all the more striking from the absolute contrast presented by the rich Neolithic cave burials a little further up the same coast—how is it to be explained? The long flint knives, the bone and shell ornaments, might, indeed, find partial parallels among Neolithic remains; but does not, after all, the balance of comparison incline to that more ancient group belonging to the "Reindeer Period" in the South of France, as illustrated by the caves of La Madeleine, Les Eyzies and Solutré?

It is true that, in an account of the interments found in 1892 in the Barma Grande Cave, given by me to the Anthropological Institute, I was myself so prepossessed by the still dominant doctrine that the usage of burial was unknown to Palæolithic man, and so overpowered by the vision of the yawning hiatus between him and his Neolithic successor, that I failed to realise the full import of the evidence. On that occasion I took refuge in the suggestion that we had here to deal with an earlier Neolithic stratum than any hitherto recorded. "Neolithic," that is, without the Neolithic.

But the accumulation of fresh data, and especially the critical observations of M. d'Acy and Prof. Issel, have convinced me that this intermediate position is untenable. From the great depth below the original surface, of what in all cases seem to have been homogeneous quaternary deposits, at which the human remains were found, it is necessary to suppose, if the interments took place at a later period, that pits in many cases from 30 to 40 feet deep must have been excavated in the cave earth. But nothing of the kind has been detected, nor any intrusion of extraneous materials. On the other hand, the gnawed or defective condition of the extremities in several cases points clearly to superficial and imperfect interment of the body; and in one case parts of the same core from which flints found with the skeleton had been chipped were found some metres distant on the same floor level. Are we, then, to imagine that another pit was expressly dug to bury these?

In the case of a more recently discovered and as yet unpublished interment, at the excavation of which I was so fortunate as to assist, the superficial character of the deposit struck the eye. The skeleton, with flint knife and ochre near, decked out with the usual shell and deer's tooth ornaments, lay as if in the attitude of sleep, somewhat on the left side. The middle of the body was covered with a large flat stone, with two smaller ones lying by it, while another large stone was laid over the feet. The left arm was bent under the head as if to pillow it, but the extremities of the right arm and the toes were suggestively deficient: the surface covering of big stones had not sufficiently protected them. The stones themselves seem in turn to have served as a kind of hearth, for a stratum of charred and burned bones about 45 cm. thick lay about them.

Is it reasonable to suppose that a deposit of this kind took place at the bottom of a pit over 20 feet deep, left open an indefinite time for the convenience of roasting venison at the bottom?

A rational survey of the evidence in this as in the other cases leads to the conclusion that we have to deal with surface burial, or, if that word seems too strong, with simple "seposition"—the imperfect covering with handy stones of the dead bodies as they lay in the attitude of sleep on the then floor of the cavern. In other words, they are *in situ* in a late quatenary deposit, for which Prof. Issel has proposed the name of "Meiolithic."

But if this conclusion is to hold good, we have here on the northern coast of the Mediterranean evidence of the existence of a late Palæolithic race, the essential features of which, in the opinion of most competent osteological inquirers, reappear in the Neolithic skeletons of the same Ligurian coast, and still remain characteristic of the historical Ligurian type. In other words, the "Mediterranean Race" finds its first record in the West; and its diffusion, so far from having necessarily followed the lines of later geographical divisions, may well have begun at a time when the land bridges of "Eurafrica" were still unbroken.

There is nothing, indeed, in all this to exclude the hypothesis that the original expansion took place from the East African side. That the earliest homes of primæval man lay in a warm region can hardly be doubted, and the abundant discovery by Mr. Seton Karr in Somaliland of Palæolithic implements reproducing many of the most characteristic forms of those of the grottoes of the Dordogne affords a new link of connection between the Red Sea and the Atlantic littoral.

When we recall the spontaneous artistic qualities of the ancient race which has left its records in the carvings on bone and ivory in the caves of the "Reindeer Period," this evidence of at least partial continuity on the northern shores of the Mediterranean suggests speculations of the deepest interest. Overlaid with new elements, swamped in the dull, though materially higher, Neolithic civilisation, may not the old æsthetic faculties which made Europe the earliest-known home of anything that can be called human art, as opposed to mere tools and mechanical contrivances, have finally emancipated themselves once more in the Southern regions, where the old stock most survived? In the extraordinary manifestations of artistic genius to which, at widely remote periods, and under the most diverse political conditions, the later populations of Greece and Italy have given birth, may we not be allowed to trace the re-emergence, as it were, after long underground meanderings, of streams whose upper waters had seen the daylight of that earlier world?

But the vast gulf of time beyond which it is necessary to carry back our gaze in order to establish such connections will hardly permit us to arrive at more than vague probabilities. The practical problems that concern the later culture of Europe from Neolithic times onwards connect themselves rather with its relation to that of the older civilisations on the southern and eastern Mediterranean shores.

Anthropology, too, has its "Eternal Eastern Question." Til

within quite recent years, the glamour of the Orient pervaded all inquiries as to the genesis of European civilisation. The Biblical training of the northern nations prepared the ground. The imperfect realisation of the antiquity of European art; on the other hand, the imposing chronology of Egypt and Babylonia; the abiding force of classical tradition, which found in the Phœnician a *deus ex machinâ* for exotic importations; finally, the "Aryan Hypothesis," which brought in the dominant European races as immigrant wanderers from Central Asia, with a ready-made stock of culture in their wallets—these and other causes combined to create an exaggerated estimate of the part played by the East as the illuminator of the benighted West.

More recent investigations have resulted in a natural reaction. The primitive "Aryan" can be no longer invoked as a kind of patriarchal missionary of Central Asian culture. From d'Halloy and Latham onwards to Penka and Schrader an array of eminent names has assigned to him an European origin. The means by which a kindred tongue diffused itself among the most heterogeneous ethnic factors still remain obscure; but the stricter application of phonetic laws and the increased detection of loan-words has cut down the original "Aryan" stock of culture to very narrow limits, and entirely stripped the members of this linguistic family of any trace of a common Pantheon.

Whatever the character of the original "Aryan" stage, we may be very sure that it lies far back in the mists of the European Stone Age. The supposed common names for metals prove to be either a vanishing quantity or strikingly irrelevant. It may be interesting to learn on unimpeachable authority that the Celtic words for "gold" are due to comparatively recent borrowing from the Latin; but nothing is more certain than that gold was one of the earliest metals known to the Celtic races, its knowledge going back to the limits of the pure Stone Age. We are told that the Latin "ensis," "a sword," is identical with the Sanskrit "asi" and Iranian "ahi," but the gradual evolution of the sword from the dagger, only completed at a late period of the Bronze Age, is a commonplace of prehistoric archæology. If "ensis," then, in historical times an iron sword, originally meant a bronze dagger, may not the bronze dagger in its turn resolve itself into a flint knife?

The truth is that the attempts to father on a common Aryan stock the beginnings of metallurgy argue an astonishing inability to realise the vast antiquity of languages and their groups. Yet we know that, as far back as we have any written records, the leading branches of the Aryan family of speech stood almost as far apart as they do to-day, and the example of the Egyptian and Semitic groups, which Maspero and others consider to have been originally connected, leads to still more striking results. From the earliest Egyptian stela to the latest Coptic liturgy we find the main outlines of what is substantially the same language preserved for a period of some six thousand years. The Semitic languages in their characteristic shape show a continuous history almost as extensive. For the date of the diverging point of the two groups we must have recourse to a chronology more familiar to the geologist than the antiquary.

As importer of exotic arts into primitive Europe the Phœnician has met the fate of the immigrants from the Central Asian "Arya." The days are gone past when it could be seriously maintained that the Phœnician merchant landed on the coast of Cornwall, or built the dolmens of the North and West. A truer view of primitive trade as passing on by inter-tribal barter has superseded the idea of a direct commerce between remote localities. The science of prehistoric archæology, following the lead of the Scandinavian School, has established the existence in every province of local centres of early metallurgy, and it is no longer believed that the implements and utensils of the European Bronze Age were imported wholesale by Semites or "Etruscans."

It is, however, the less necessary for me to trace in detail the course of this reaction against the exaggerated claims of Eastern influence that the case for the independent position of primitive Europe has been recently summed up with fresh arguments, and in his usual brilliant and incisive style, by M. Salomon Reinach, in his "Mirage Orientale." For many ancient prejudices as to the early relations of East and West it is the trumpet sound before the walls of Jericho. It may, indeed, be doubted whether, in the impetuousness of his attack, M. Reinach, though he has rapidly brought up his reserves in his more recent work on primitive European sculpture, has not been tempted to occupy outlying positions in the enemy's country which will hardly be found tenable in the long run. I cannot myself, for instance, be brought to believe that the rude marble "idols" of the primitive Ægean population were copied on Chaldæan cylinders. I may have occasion to point out that the oriental elements in the typical higher cultures of primitive Europe, such as those of Mycenæ, of Hallstatt, and La Tène, are more deeply rooted than M. Reinach will admit. But the very considerable extent to which the early European civilisation was of independent evolution has been nowhere so skilfully focussed into light as in these comprehensive essays of M. Reinach. It is always a great gain to have the extreme European claims so clearly formulated, but we must still remember that the "Sick Man" is not dead.

The proofs of a highly developed metallurgic industry of home growth accumulated by prehistoric students *pari passu* over the greater part of Europe, and the considerable cultural equipment of its early population—illustrated, for example, in the Swiss Lake settlements—had already prepared the way for the more startling revelations as to the prehistoric civilisation of the Ægean world which have resulted from Dr. Schliemann's diggings at Troy, Tiryns, and Mycenæ, so admirably followed up by Dr. Tsountas.

This later civilisation, to which the general name of "Ægean" has been given, shows several stages, marked in succession by typical groups of finds, such as those from the Second City of Troy, from the cist-graves of Amorgos, from beneath the volcanic stratum of Thera, from the shaft-graves of Mycenæ, and again from the tombs of the lower town. Roughly, it falls into two divisions, for the earlier of which the culture illustrated by the remains of Amorgos may be taken as the culminating point, while the later is inseparably connected with the name of Mycenæ.

The early "Ægean" culture rises in the midst of a vast province extending from Switzerland and Northern Italy through the Danubian basin and the Balkan peninsula, and continued through a large part of Anatolia, till it finally reaches Cyprus. It should never be left out of sight that, so far as the earliest historical tradition and geographical nomenclature reach back, a great tract of Asia Minor is found in the occupation of men of European race, of whom the Phrygians and their kin—closely allied to the Thracians on the other side of the Bosphorus—stand forth as the leading representatives. On the other hand, the great antiquity of the Armenoid type in Lycia and other easterly parts of Asia Minor, and its priority to the Semites in these regions, has been demonstrated by the craniological researches of Dr. von Luschan. This ethnographic connection with the European stock, the antiquity of which is carried back by Egyptian records to the second millennium before our era, is fully borne out by the archæological evidence. Very similar examples of ceramic manufactures recur over the whole of this vast region. The resemblances extend even to minutiæ of ornament, as is well shown by the examples compared by Dr. Much from the Mondsee, in Upper Austria, from the earliest stratum of Hissarlik, and from Cyprus. It is in the same Anatolo-Danubian area—as M. Reinach has well pointed out—that we find the original centre of diffusion of the "Svastika" motive in the Old World. Copper implements, and weapons, too, of primitive types, some reproducing Neolithic forms, are also a common characteristic, though it must always be remembered that the mere fact that an implement is of copper does not of itself necessitate its belonging to the earliest metal age, and that the freedom from alloy was often simply due to a temporary deficiency of tin. Cyprus, the land of copper, played, no doubt, a leading part in the dissemination of this early metallurgy, and certain typical pins and other objects found in the Alpine and Danubian regions have been traced back by Dr. Naue and others to Cypriote prototypes. The same parallelism throughout this vast area comes out again in the appearance of a class of primitive "idols" of clay, marble, and other materials, extending from Cyprus to the Troad and the Ægean islands, and thence to the pile settlements of the Alps and the Danubian basin, while kindred forms can be traced beyond the Carpathians to a vast northern Neolithic province that stretches to the shores of Lake Ladoga.

It is from the centre of this old Anatolo-Danubian area of primitive culture, in which Asia Minor appears as a part of Europe, that the new Ægean civilisation rises from the sea. "Life was stirring in the waters." The notion that the maritime enterprise of the Eastern Mediterranean began on the exposed and comparatively harbourless coast of Syria and Palestine can

no longer be maintained. The island world of the Ægean was the natural home of primitive navigation. The early sea-trade of the inhabitants gave them a start over their neighbours, and produced a higher form of culture, which was destined to react on that of a vast European zone—nay, even upon that of the older civilisations of Egypt and Asia.

The earlier stage of this Ægean culture culminates in what may conveniently be called the Period of Amorgos from the abundant tombs explored by Dr. Dümmler and others in that island. Here we already see the proofs of a widespread commerce. The ivory ornaments point to the South; the abundance of silver may even suggest an intercourse along the Libyan coast with the rich silver-producing region of South-eastern Spain, the very ancient exploitation of which has been so splendidly illustrated by the researches of the brothers Siret. Additional weight is lent to this presumption by the recurrence in these Spanish deposits of pots with rude indications of eyes and eyebrows, recalling Schliemann's owl-faced urns; of stone "idols," practically identical with those of Troy and the Ægean islands, here too associated with marble cups of the same simple forms; of triangular daggers of copper and bronze, and of bronze swords which seem to stand in a filial relation to an "Amorgan" type of dagger. In a former communication to this Section I ventured to see in the so-called "Cabiri" of Malta—very far removed from any Phœnician sculpture—an intermediate link between the Iberian group and that of the Ægean, and to trace on the fern-like ornaments of the altar-stone a comparison with the naturalistic motives of proto-Mycenæan art, as seen, for instance, on the early vases of Thera and Therasia.

A Chaldæan influence cannot certainly be excluded from this early Ægean art. It reveals itself, for instance, in indigenous imitations of Babylonian cylinders. My own conclusion that the small marble figures of the Ægean deposits, though of indigenous European lineage, were in their more developed types influenced by Istar models from the East, has since been independently arrived at by the Danish archæologist, Dr. Blinkenburg, in his study on præ-Mycenæan art.

More especially the returning-spiral decoration, which in the "Amorgan Period" appears upon seals, rings, bowls, and caskets of steatite, leads us to a very interesting field of comparison. This motive, destined to play such an important part in the history of European ornament, is absent from the earlier products of the great Anatolo-Danubian province. As a European design it is first found on these insular fabrics, and it is important to observe that it first shows itself in the form of reliefs on stone. The generally accepted idea, put forward by Dr. Milchhöfer, that it originated here from applied spirals on metal work is thus seen to be bereft of historical justification. At a somewhat later date we find this spiraliform motive communicating itself to the ceramic products of the Danubian region, though from the bold relief in which it sometimes appears, a reminiscence of the earlier steatite reliefs seems still traceable. In the late Neolithic pile-station of Butmir, in Bosnia, this spiral decoration appears in great perfection on the pottery, and is here associated with clay images of very advanced fabric. At Lengyel, in Hungary, and elsewhere, we see it applied to primitive painted pottery. Finally, in the later Hungarian Bronze Age it is transferred to metal work.

But this connection—every link of which can be made out—of the lower Danubian Bronze Age decoration with the Ægean spiral system—itself much earlier in origin—has a very important bearing on the history of ornament in the North and West. The close relation of the Bronze Age culture of Scandinavia and North-western Germany with that of Hungary is clearly established, and of the many valuable contributions made by Dr. Montelius to prehistoric archæology, none is more brilliant than his demonstration that this parallelism of culture between the North-west and South-east owes its origin to the most ancient course of the amber trade from the North Sea shores of Jutland by the valley of the Elbe and Moldau to the Danubian Basin. As Dr. Montelius has also shown, there was, besides, a western extension of this trade to our own islands. If Scandinavia and its borderlands were the source of amber, Ireland was the land of gold. The wealth of the precious metal there is illustrated by the fact that, even as late as 1796, the gold washings of County Wicklow amounted to 10,000*l*. A variety of evidence shows a direct connection between Great Britain and Scandinavia from the end of the Stone Age onwards. Gold diadems of unquestionably British—probably Irish—fabric have been found in Seeland and Fünen, and from the analysis of early gold ornaments it clearly results that it was from Ireland rather than the Ural that Northern and Central Europe was supplied. Mr. Coffey, who has made an exhaustive study of the early Irish monuments, has recently illustrated this early connection by other comparisons, notably the appearance of a design which he identifies with the early carvings of boats on the rocks of Scandinavia.

This prolongation of the Bronze Age trade route—already traced from the Middle Danube—from Scandinavia to Ireland, ought it to be regarded as the historic clue to the contemporary appearance of the spiral motive in the British Islands? Is it to this earlier intercourse with the land of the Vikings that we must ascribe the spiral scrolls on the slabs of the great chambered barrows of the Irish Bronze Age—best seen in the most imposing of them all, before the portal and on the inner chambers of New Grange?

The possibility of such a connection must be admitted; the probability is great that the contemporary appearance of the spiraliform ornament in Ireland and on the continent of Europe is due to direct derivation. It is, of course, conceivable that such a simple motive as the returning spiral may have originated independently in various parts of Europe, as it did originate in other parts of the world. But anthropology has ceased to content itself with the mere accumulation of sporadic coincidences. It has become a historic study. It is not sufficient to know how such and such phenomena *may* have originated, but how, as a matter of fact, they *did*. Hence in the investigation of origins and evolution the special value of the European field where the evidence has been more perfectly correlated and the continuous records go further back. An isolated example of the simple volute design belonging to the "Reindeer Period" has been found in the grotto of Arudy. But the earliest cultural strata of Europe, from the beginning of the Neolithic period onwards, betray an entire absence of the returning spiral motive. When we find it later propagating itself as a definite ornamental system in a regular chronological succession throughout an otherwise inter-related European zone, we have every right to trace it to a common source.

But it does not therefore follow that the only alternative is to believe that the spiral decoration of the Irish monuments necessarily connects itself with the ancient stream of intercourse flowing from Scandinavia.

We have to remember that the Western lands of gold and tin were the goals of other prehistoric routes. Especially must we bear in mind the early evidence of intercourse between the British Isles and the old Iberic region of the opposite shores of the continent. The derivation of certain forms of Bronze Age types in Britain and Ireland from this side has already been demonstrated by my father, and British or Irish bronze flat axes with their characteristic ornamentation have in their turn been found in Spain as well as in Denmark. The peculiar technique of certain Irish flint arrowheads of the same period, in which chipping and grinding are combined, is also characteristic of the Iberian province, and seems to lead to very extended comparisons on the Libyan side, recurring as it does in the exquisite handiwork of the non-Egyptian race whose relics Mr. Petrie has brought to light at Nagada. In prehistoric Spanish deposits, again, are found the actual wallet-like baskets with incurving sides, the prototypes of a class of clay food-vessels which (together with a much wider distribution) are of specially frequent occurrence in the British Isles as well as the old Iberian area.

If the spiral decoration had been also a feature of the Scandinavian rock carvings, the argument for derivation from that side would have been strong. But they are not found in them, and, on the other hand, the sculptures on the dolmens of the Morbihan equally show certain features common to the Irish stone chambers, including the primitive ship figure. The spiral itself does not appear on these; but the more the common elements between the Megalithic piles, not only of the old Iberian tract on the mainland, including Brittany, but in the islands of the West Mediterranean basin, are realised, the more probable it becomes that the impulse came from this side. The prehistoric buildings of Malta, hitherto spoken of as "Phœnician temples," which show in their primitive conception a great affinity to the Megalithic chambers of the earliest British barrows, bear witness on this side to the extension of the Ægean spiral system in a somewhat advanced stage, and accompanied, as at New Grange, with intermediate lozenges. In Sardinia, as I hope to show, there is evidence of the former existence of monuments of Mycenæan architecture in which the

chevron, the lozenge, and the spiral might have been seen associated as in Ireland. It is on this line, rather than on the Danube and the Elbe, that we find in a continuous zone that Cyclopean tradition of domed chambers which is equally illustrated at Mycenæ and at New Grange.

These are not more than indications, but they gain additional force from the converging evidence to which attention has already been called of an ancient line of intercourse, mainly, we may believe, connected with the tin trade between the East Mediterranean basin and the Iberian West. A further corroboration of the view that an Ægean impulse propagated itself as far as our own islands from that side is perhaps afforded by a very remarkable find in a British barrow.

I refer to the Bronze Age interment excavated by Canon Greenwell on Folkton Wold, in Yorkshire, in which, beside the body of a child, were found three carved chalk objects resembling round boxes with bossed lids. On one of these lids were grouped together, with a lozenge-shaped space between them, two partly spiraliform partly concentric circular ornaments, which exhibit before our eyes the degeneration of two pairs of returning spiral ornaments. Upon the sides of two of these chalk caskets, associated with chevrons, saltires, and lozenges, were rude indications of faces—eyes and nose of bird-like character—curiously recalling the early Ægean and Trojan types of Dr. Schliemann. These, as M. Reinach has pointed out, also find an almost exact parallel in the rude indications of the human face seen on the sculptured menhirs of the Marne and the Gard valleys. To this may be added the interesting comparisons supplied by certain clay vessels, of rounded form, somewhat resembling the chalk "caskets" discovered by MM. Siret in Spanish interments of the early metal age, in which eyes and eyebrows of a primitive style are inserted, as on the British relics, in the interspaces of linear ornamentation. The third chalk disc exhibits, in place of the human face, a butterfly with volute antennæ, reminding us of the appearance of butterflies as a decorative motive on the gold roundels from the shaft-graves of Mycenæ, as also on early Mycenæan gems of steatite from Crete; in the latter case with the feelers curving outwards in the same way. The stellate design with central circles on the lid of one of the chalk caskets is itself not impossibly a distant degeneration of the star-flowers on the same Mycenæan plates. Putting all these separate elements of resemblance together—the returning spiral and star, the rude face and butterfly—the suggestion of Ægean reminiscence becomes strong, but the other parallels lead us for the line of its transmission towards the Iberian rather than the Scandinavian route.[1]

So much, at least, results from these various comparisons that, whether we find the spiral motive in the extreme West or North of Europe, everything points to the Ægean world as its first European centre. But have we any right to regard it, even there, as of indigenous evolution?

It had been long my own conviction that the Ægean spiral system must itself be regarded as an offshoot of that of ancient Egypt, which as a decorative motive on scarabs goes back, as Prof. Petrie has shown, to the Fourth Dynasty. During the time of the Twelfth Dynasty, which, on general grounds, may be supposed roughly to correspond with the "Amorgan Period" of Ægean culture, it attained its apogee. The spiral convolutions now often cover the whole field of the scarab, and the motive begins to spread to a class of black bucchero vases, the chalk inlaying of whose ornaments suggests widespread European analogies. But the important feature to observe is that here, as in the case of the early Ægean examples, the original material on which the spiral ornament appears is stone, and that, so far from being derived from an advanced type of metal work, it goes back in Egypt to a time when metal was hardly known.

The prevalence of the spiral ornamentation on stone work in the Ægean islands and contemporary Egypt, was it merely to be regarded as a coincidence? To turn one's eye to the Nile Valley, was it simply another instance of the "*Mirage Orientale*"? For my own part, I ventured to believe that, as in the case of Northern Europe, the spread of this system was connected with many collateral symptoms of commercial interconnection, so here, too, the appearance of this early Ægean ornament would be found to lead to the demonstration of a direct intercourse between the Greek islands and Egypt at least a thousand years earlier than any that had hitherto been allowed.

One's thoughts naturally turned to Crete, the central island, with one face on the Libyan Sea—the natural source and seminary of Ægean culture—where fresh light was already being thrown on the Mycenæan civilisation by the researches of Prof. Halbherr, but the earlier prehistoric remains of which were still unexplored. Nor were these expectations unfounded. As the result of three expeditions—undertaken in three successive years, from the last of which I returned three months since—it has been my fortune to collect a series of evidences of a very early and intimate contact with Egypt, going back at least to the Twelfth Dynasty, and to the earlier half of the third millennium before our era. It is not only that in primitive deposits, like that of Hagios Onuphrios, scarabs, acknowledged by competent archæologists to be of Twelfth Dynasty date, occurred in association with steatite seals presenting the Ægean spiral ornamentation, and with early pottery answering to that of Amorgos and the second city of Troy. This by itself might be regarded by many as convincing. But—what from the point of view of intercourse and chronology is even more important—in the same deposit and elsewhere occurred early button-shaped and triangular seals of steatite with undoubted indigenous copies of Egyptian lotos designs characteristic of the same period, while in the case of the three-sided bead-seals it was possible to trace a regular evolution leading down to Mycenæan times. Nor was this all. Throughout the whole of the island there came to light a great variety of primitive stone vases, mostly of steatite, a large proportion of which reproduced the characteristic forms of Egyptian stone vases, in harder materials, going far back into the Ancient Empire. The returning spiral motive is also associated with these, as may be seen from a specimen now in the collection of Dr. Naue, of Munich.

A geological phenomenon which I was able to ascertain in the course of my recent exploration of the eastern part of the island goes far to explain the great importance which these steatite or "soapstone" fabrics played in the primitive culture of Crete and the Ægean islands. In the valley of the Sarakina stream I came upon vast deposits of this material, the diffusion of which could be further traced along a considerable tract of the southern coast. The abundant presence of this attractive and, at the same time, easily workable stone—then incomparably more valuable, owing to the imperfection of the potter's art—goes far to explain the extent to which these ancient Egyptian forms were imitated, and the consequent spread of the returning spiral motive throughout the Ægean.

In the matter of the spiral motive, Crete may thus be said to be the missing link between prehistoric Ireland and Scandinavia and the Egypt of the Ancient Empire. But the early remains of the island illustrate in many other ways the comparatively high level of culture already reached by the Ægean population in præ-Mycenæan times. Especially are they valuable in supplying the antecedent stages to many characteristic elements of the succeeding Mycenæan civilisation.

This ancestral relationship is nowhere more clearly traceable than in a class of relics which bear out the ancient claim of the islanders that they themselves had invented a system of writing to which the Phœnicians did not do more than add the finishing touches. Already, at the Oxford meeting of the Association, I was able to call attention to the evidence of the existence of a prehistoric Cretan script evolved by gradual simplification and selection from an earlier picture writing. This earlier stage is, roughly speaking, illustrated by a series of primitive seals belonging to the "Period of Amorgos." In the succeeding Mycenæan age the script is more conventionalised, often linear, and though developments of the earlier forms of seals are frequently found, they are usually of harder materials, and the system is applied to other objects. As the result of my most recent investigations, I am now able to announce the discovery of an inscribed prehistoric relic, which surpasses in interest and importance all hitherto known objects of this class. It consists of a fragment of what may be described as a steatite "Table of Offerings," bearing part of what appears to be a dedication of nine letters of probably syllabic values, answering to the same early Cretan script that is seen on the seals, and with two punctuations. It was obtained from the lowest level of a

[1] A further piece of evidence pointing in this direction is supplied by one of the chalk "caskets." On the upper disc of this, in the place corresponding with the double-spirals on the other example, appears a degeneration of the same motive in a more compressed form, resembling two sets of concentric horseshoes united at their bases. This recurs at New Grange, and single sets of concentric horseshoes, or semicircles, are found both there and at Gavrinnis. The degeneration of the returning spiral motive extends therefore to Brittany.

Mycenæan stratum, containing numerous votive objects, in the great cave of Mount Dikta, which, according to the Greek legend, was the birthplace of Zeus.

This early Cretan script, which precedes by centuries the most ancient records of Phœnician writing, and supplies, at any rate, very close analogies to what may be supposed to have been the pictorial prototypes of several of the Phœnician letters, stands in a direct relation to the syllabic characters used at a later date by the Greeks of Cyprus. The great step in the history of writing implied by the evolution of symbols of phonetic value from primitive pictographs is thus shown to have effected itself on European soil.

In many other ways the culture of Mycenæ—that extraordinary revelation from the soil of prehistoric Greece—can be shown to be rooted in this earlier Ægean stratum. The spiral system, still seen in much of its pure original form on the gold vessels and ornaments from the earlier shaft-graves of Mycenæ, is simply the translation into metal of the pre-existing steatite decoration. (See *Hellenic Journal*, xii., 1892, p. 221.)

The Mycenæan repoussé work in its most developed stage as applied to human and animal subjects has probably the same origin in stone work. Cretan examples, indeed, give the actual transition in which an intaglio in dark steatite is coated with a thin gold plate impressed into the design. On the other hand, the noblest of all creations of the Mycenæan goldsmith's art, the Vaphio cups, with their bold reliefs, illustrating the hunting and capture of wild bulls, find their nearest analogy in a fragment of a cup, procured by me from Knôsos, of Black Cretan steatite, with naturalistic reliefs, exhibiting a fig-tree in a sacred enclosure, an altar, and men in high action, which in all probability was originally coated, like the intaglio, with thin plates of gold.

In view of some still prevalent theories as to the origin of Mycenæan art, it is important to bear in mind these analogies and connections, which show how deeply set its roots are in Ægian soil. The Vaphio cups, especially, from their superior art, have been widely regarded as of exotic fabric. That the art of an European population in prehistoric times should have risen above that of contemporary Egypt and Babylonia was something beyond the comprehension of the traditional school. These most characteristic products of indigenous skill, with their spirited representations of a sport the traditional home of which in later times was the Thessalian plains, have been, therefore, brought from "Northern Syria"! Yet a whole series of Mycenæan gems exists executed in the same bold naturalistic style, and of local materials, such as lapis Lacedæmonius, the subjects of which are drawn from the same artistic cycle as those of the cups, and not one of these has as yet been found on the Eastern Mediterranean shores. Like the other kindred intaglios, they all come from the Peloponnese, from Crete, from the shores and islands of the Ægean, from the area, that is, where their materials were procured. Their lentoid and almond-shaped forms are altogether foreign to Semitic usage, which clung to the cylinder and cone. The finer products of the Mycenæan glyptic art on harder materials were, in fact, the outcome of long apprentice studies of the earlier Ægean population, of which we have now the record in the primitive Cretan seals, and the explanation in the vast beds of such an easily worked material as steatite.

But the importation of the most characteristic Mycenæan products from "Northern Syria" has become quite a moderate proposition beside that which we have now before us. In a recent communication to the French Academy of Inscriptions, Dr. Helbig has reintroduced to us as a more familiar figure. Driven from his prehistoric haunts on the Atlantic coasts, torn from the Cassiterides, dislodged even from his Thucididean plantations in pre-Hellenic Sicily, the Phœnician has returned, tricked out as the true "Mycenæan."

A great part of Dr. Helbig's argument has been answered by anticipation. Regardless of the existence of a regular succession of intermediate glyptic types, such as the "Melian" gems and the engraved seals of the geometrical deposits of the Greek mainland, like those of Olympia and of the Heræon at Argos, which link the Mycenæan with the classical series, Dr. Helbig takes a verse of Homer to hang from it a theory that seals and engraved stones were unknown to the early Greeks. On this imaginary fact he builds the astounding statement that the engraved gems and seals found with Mycenæan remains must be of foreign and, as he believes, Phœnician importation. The stray diffusion of one or two examples of Mycenæan pots to the coast of Palestine, the partial resemblance of some Hittite bronze figures, executed in a more barbarous Syrian style, to specimens of quite different fabric found at Tiryns, Mycenæ, and, it may be added, in a Cretan cave near Sybrita, the wholly unwarranted attribution to Phœnicia of a bronze vase-handle found in Cyprus, exhibiting the typical lion-headed demons of the Mycenæans—these are only a few salient examples of the reasoning by which the whole prehistoric civilisation of the Greek world, so instinct with naturalism and individuality, is handed over to the least original member of the Semitic race. The absence in historic Greece of such arts as that of *intarsia* in metal work, of glass-making (if true) and of porcelain-making, is used as a conclusive argument against their practice by an Ægean population, of uncertain stock, a thousand years earlier, as if in the intervening dark ages between the primitive civilisation of the Greek lands and the Classical Renaissance no arts could have been lost!

Finally, the merchants of Keftô depicted on the Egyptian monuments are once more claimed as Phœnicians, and with them—though this is by no means a necessary conclusion, even from the premise—the precious gifts they bear, including vases of characteristic Mycenæan form and ornament. All this is diametrically opposed to the conclusions of the most careful inquirer into the origins of this mysterious people, Dr. W. Max Müller (to be distinguished from the eminent Professor), who shows that the list of countries in which Keftô occurs places them beyond the limit of Phœnicia or of any Semitic country, and connects them rather with Cilicia and with Cyprus, the scene, as we now know, of important Mycenæan plantations. It is certain that not only do the Keftiu traders bear articles of Mycenæan fabric, but their costume, which is wholly un-Semitic, their leggings and sandals, and the long double locks of hair streaming down below their armpits, identify them with the men of the frescoes of Mycenæ, and of the Vaphio and Knôsian cups.

The truth is that these Syrian and Phœnician theories are largely to be traced to the inability to understand the extent to which the primitive inhabitants of the Ægean shores had been able to assimilate exotic arts without losing their own individuality. The precocious offspring of our continent, first come to man's estate in the Ægean island world, had acquired cosmopolitan tastes, and already stretched forth his hands to pluck the fruit of knowledge from Oriental boughs. He had adopted foreign fashions of dress and ornament. His artists revelled in lion hunts and palm-trees. His very worship was infected by the creations of foreign religions.

The great extent to which the Mycenæans had assimilated exotic arts and ideas can only be understood when it is realised that this adaptive process had begun at least a thousand years before, in the earlier period of Ægean culture. New impulses from Egypt and Chaldæa now succeed the old. The connection with Eighteenth and Nineteenth Dynasty Egypt was of so intimate a kind that it can only be explained by actual settlement from the Ægean side. The abundant relics of Ægean ceramic manufactures found by Prof. Petrie on Egyptian sites fully bear out this presumption. The early marks on potsherds discovered by that explorer seem to carry the connection back to the earlier Ægean period, but the painted pottery belongs to what may broadly be described as Mycenæan times. The earliest relics of this kind found in the rubbish heaps of Kahun, though it can hardly be admitted that they go quite so far back as the Twelfth Dynasty date assigned to them by Mr. Petrie (*c.* 2500 B.C.), yet correspond with the earliest Mycenæan classes found at Thera and Tiryns, and seem to find their nearest parallels in pottery of the same character from the cave of Kamares on the northern steep of the Cretan Ida, recently described by Mr. J. L. Myres and by Dr. Lucio Mariani. Vases of the more typical Mycenæan class have been found by Mr. Petrie in a series of deposits dated, from the associated Egyptian relics, from the reign of Thothmes III. onwards (1450 B.C.). There is nothing Phœnician about these—with their seaweeds and marine creatures they are the true products of the island world of Greece. The counterpart to these Mycenæan imports in Egypt is seen in the purely Egyptian designs which now invade the northern shores of the Ægean, such as the ceiling of the sepulchral chamber at Orchomenos, or the wall-paintings of the palace at Tiryns—almost exact copies of the ceilings of the Theban tombs—designs distinguished by the later Egyptian combination of the spiral and plant ornament which at this period supersedes the pure returning spiral of the earlier dynasties. The same contemporary

evidence of date is seen in the scarabs and porcelain fragments with the cartouches of Queen Tyi and Amenhotep III., found in the Mycenæan deposits. But more than a mere commercial connection between the Ægean seat of Mycenæan culture and Egypt seems to be indicated by some of the inlaid daggers from the Acropolis tombs. The subject of that representing the ichneumons hunting ducks amidst the lotos thickets beside a stream that can only be the Nile, as much as the intarsia technique, is so purely of Egypt that it can only have been executed by a Mycenæan artificer resident within its borders. The whole cycle of Egyptian Nile-pieces thoroughly penetrated Mycenæan art—the duck-catcher in his Nile-boat, the water-fowl and butterflies among the river-plants, the spotted cows and calves, supplied fertile motives for the Mycenæan goldsmiths and ceramic artists. The griffins of Mycenæ reproduce an elegant creation of the New Empire, in which an influence from the Asiatic side is also traceable.

The assimilation of Babylonian elements was equally extensive. It, too, as we have seen, had begun in the earlier Ægean period, and the religious influence from the Semitic side, of which traces are already seen in the assimilation of the more primitive "idols" to Eastern models, now forms a singular blend with the Egyptian, as regards, at least, the externals of cult. We see priests, in long folding robes of Asiatic cut, leading griffins, offering doves, holding axes of a type of Egyptian derivation which seems to have been common to the Syrian coast, the Hittite regions of Anatolia, and Mycenæan Greece. Female votaries in flounced Babylonian dresses stand before seated Goddesses, rays suggesting those of Shamas shoot from a Sun-God's shoulders, conjoined figures of moon and star recall the symbols of Sin and Istar, and the worship of a divine pair of male and female divinities is widely traceable, reproducing the relations of a Semitic Bel and Beltis. The cylinder subjects of Chaldæan art continually assert themselves: a Mycenæan hero steps into the place of Gilgames or Eabani, and renews their struggles with wild beasts and demons in the same conventional attitudes, of which Christian art has preserved a reminiscence in its early type of Daniel in the lions' den. The peculiar schemes resulting from, or, at least, brought into continual prominence by the special conditions of cylinder engraving, with the constant tendency to which it is liable of the two ends of the design to overlap, deeply influenced the glyptic style of Mycenæ. Here, too, we see the same animals with crossed bodies, with two bodies and a single head, or simply confronted. These latter affiliations to Babylonian prototypes have a very important bearing on many later offshoots of European culture. The tradition of these heraldic groups preserved by the later Mycenæan art, and communicated by it to the so-called "Oriental" style of Greece, finds in another direction its unbroken continuity in ornamental products of the Hallstatt province, and that of the late Celtic metal workers.

"But this," exclaims a friendly critic, "is the old heresy—the '*Mirage Orientale*' over again. Such heraldic combinations have originated independently elsewhere:—why may they not be of indigenous origin in primitive Europe?"

They certainly may be. Confronted figures occur already in the Dordogne caves. But, in a variety of instances, the historic and geographical connection of these types with the Mycenæan, and those in turn with the Oriental, is clearly made out. That system which leaves the least call on human efforts at inventiveness seems in anthropology to be the safest.

Let us then fully acknowledge the indebtedness of early Ægean culture to the older civilisations of the East. But this indebtedness must not be allowed to obscure the fact that what was borrowed was also assimilated. On the easternmost coast of the Mediterranean, as in Egypt, it is not in a pauper's guise that the Mycenæan element makes its appearance. It is rather the invasion of a conquering and superior culture. It has already outstripped its instructors. In Cyprus, which had lagged behind the Ægean peoples in the race of progress, the Mycenæan relics make their appearance as imported objects of far superior fabric, side by side with the rude insular products. The final engrafting on Cypriote soil of what may be called a colonial plantation of Mycenæ later reacts on Assyrian art, and justifies the bold theory of Prof. Brunn that the sculptures of Nineveh betray Greek handiwork. The concordant Hebrew tradition that the Philistines were immigrants from the Islands of the Sea, the name "Cherethim," or Cretans, actually applied to them, and the religious ties which attached "Minoan" Gaza to the cult of the Cretan Zeus, are so many indications that the Ægean settlements, which in all probability existed in the Delta, extended to the neighbouring coast of Canaan, and that amongst other towns the great staple of the Red Sea trade had passed into the hands of these prehistoric Vikings. The influence of the Mycenæans on the later Phœnicians is abundantly illustrated in their eclectic art. The Cretan evidence tends to show that even the origins of their alphabet receive illustration from the earlier Ægean pictography. It is not the Mycenæans who are Phœnicians. It is the Phœnicians who, in many respects, acted as the depositaries of decadent Mycenæan art.

If there is one thing more characteristic than another of Phœnician art, it is its borrowed nature, and its incongruous collocation of foreign elements. Dr. Helbig himself admits that if Mycenæan art is to be regarded as the older Phœnician, the Phœnician historically known to us must have changed his nature. What the Mycenæans took they made their own. They borrowed from the designs of Babylonian cylinders, but they adapted them to gems and seals of their own fashion, and rejected the cylinders themselves. The influence of Oriental religious types is traceable on their signet rings, but the liveliness of treatment and the dramatic action introduced into the groups separate them, *toto cælo*, from the conventional schematism of Babylonian cult-scenes. The older element, the sacred trees and pillars which appear as the background of these scenes—on this I hope to say more later on in this Section—there is no reason to regard here as Semitic. It belongs to a religious stage widely represented on primitive European soil, and nowhere more persistent than in the West.

Mycenæan culture was permeated by Oriental elements, but never subdued by them. This independent quality would alone be sufficient to fix its original birthplace in an area removed from immediate contiguity with that of the older civilisations of Egypt and Babylonia. The Ægean island world answers admirably to the conditions of the case. It is near, yet sufficiently removed, combining maritime access with insular security. We see the difference if we compare the civilisation of the Hittites of Anatolia and Northern Syria, in some respects so closely parallel with that of Mycenæ. The native elements were there cramped and trammelled from the beginning by the Oriental contact. No real life and freedom of expression was ever reached; the art is stiff, conventional, becoming more and more Asiatic, till finally crushed out by Assyrian conquest. It is the same with the Phœnicians. But in prehistoric Greece the indigenous element was able to hold its own, and to recast what it took from others in an original mould. Throughout its handiwork there breathes the European spirit of individuality and freedom. Prof. Petrie's discoveries at Tell-el-Amarna show the contact of this Ægean element for a moment infusing naturalism and life into the time-honoured conventionalities of Egypt itself.

A variety of evidence, moreover, tends to show that during the Mycenæan period the earlier Ægean stock was reinforced by new race elements coming from north and west. The appearance of the primitive fiddle-bow-shaped *fibula* or safety-pin brings Mycenæan Greece into a suggestive relation with the Danube Valley and the Terremare of Northern Italy. Certain ceramic forms show the same affinities; and it may be noted that the peculiar "two-storied" structure of the "Villanova" type of urn which characterises the earliest Iron Age deposits of Italy finds already a close counterpart in a vessel from an Akropolis grave at Mycenæ—a parallelism which may point to a common Illyrian source. The painted pottery of the Mycenæans itself, with its polychrome designs, betrays Northern and Western affinities of a very early character, though the glaze and exquisite technique were doubtless elaborated in the Ægean shores. Examples of spiraliform painted designs on pottery going back to the borders of the Neolithic period have been found in Hungary and Bosnia. In the early rock-tombs of Sicily of the period anterior to that marked by imported products of the fully developed Mycenæan culture are found unglazed painted wares of considerable brilliancy, and allied classes recur in the heel of Italy and in the cave deposits of Liguria of the period transitional between the use of stone and metal. The "household gods," if so we may call them, of the Mycenæans also break away from the tradition of the marble Ægean forms. We recognise the coming to the fore again of primitive European clay types in a more advanced technique. Here, too, the range of comparison takes us to the same Northern and Western area. Here, too, in Sicily and Liguria, we see the primitive art of ceramic painting already applied to these at the close of the Stone Age. A rude female clay figure found in the Arene Candide cave near Finalmarina, the upper part of the body of which, armless and rounded is

painted with brown stripes on a pale rose ground, seems to me to stand in a closer relation to the prototype of a well-known Mycenæan class than any known example. A small painted image, with punctuated cross-bands over the breast, from a sepulchral grotto at Villafrati, near Palermo, belongs to the same early family as the *bucchero* types of Butmir, in Bosnia. Unquestionable parallels to the Mycenæan class have been found in early graves in Servia, of which an example copied by me some years since in the museum at Belgrade was found near the site of that later emporium of the Balkan trade, Viminacium, together with a cup attesting the survival of the primitive Ægean spirals. These extensive Italian and Illyrian comparisons, which find, perhaps, their converging point in the North-Western corner of the Balkan peninsula, show, at least approximately, the direction from which this new European impulse reached the Ægean shores.

It is an alluring supposition that this North-Western infusion may connect itself with the spread of the Greek race in the Ægean islands and the Southern part of the Balkan peninsula. There seems, at least, to be a reasonable presumption in favour of this view. The Mycenæan tradition, which underlies so much of the classical Greek art, is alone sufficient to show that a Greek element was at least included in the Mycenæan area of culture. Recent criticism has found in the Mycenæan remains the best parallel to much of the early arts and industries recorded by the Homeric poems. The *megaron* of the palaces at Tiryns and Mycenæ is the hall of Odysseus; the inlaid metal work of the shield of Achilles recalls the Egypto-Mycenæan intarsia of the dagger blades; the cup of Nestor with the feeding doves, the subjects of the ornamental design—the siege-piece, the lion-hunt, the hound with its quivering quarry—all find their parallels in the works of the Mycenæan goldsmiths. The brilliant researches of Dr. Reichel may be said to have resulted in the definite identification of the Homeric body-shield with the most typical Mycenæan form, and have found in the same source the true explanation of the greaves and other arms and accoutrements of the epic heroes.

That a Greek population shared in the civilisation of Mycenæ cannot reasonably be denied, but that is far from saying that this was necessarily the only element, or even the dominant element. Archæological comparisons, the evidence of geographical names and consistent tradition, tend to show that a kindred race, represented later by the Phrygians on the Anatolian side, the race of Pelops and Tantalos, the special votaries of Kybelê, played a leading part. In Crete a non-Hellenic element, the Eteocretes, or "true Cretans," the race of Minôs, whose name is bound up with the earliest sea-empire of the Ægean, and perhaps identical with that of the Minyans of continental Greece, preserved their own language and nationality to the borders of the classical period. The Labyrinth itself, the double-headed axe as a symbol of the divinity called Zeus by the Greek settlers, the common forms in the characters of the indigenous script, local names and historical traditions, further connect these Mycenæan aborigines of Crete with the primitive population, it, too, of European extraction, in Caria and Pisidia, and with the older elements in Lycia.

It is difficult to exaggerate the part played in this widely ramifying Mycenæan culture on later European arts from prehistoric times onwards. Beyond the limits of its original seats, primitive Greece and its islands, and the colonial plantations thrown out by it to the west coast of Asia Minor to Cyprus, and in all probability to Egypt and the Syrian coast, we can trace the direct diffusion of Mycenæan products, notably the ceramic wares, across the Danube to Transylvania and Moldavia. In the early cemeteries of the Caucasus the fibulas and other objects indicate a late Mycenæan source, though they are here blended with allied elements of a more Danubian character. The Mycenæan impress is very strong in Southern Italy, and, to take a single instance, the prevailing sword-type of that region is of Mycenæan origin. Along the western Adriatic coast the same influence is traceable to a very late date in the sepulchral stelæ of Pesaro and the tympanum relief of Bologna, and bronze knives of the prehistoric Greek type find their way into the later Terremare. At Orvieto and elsewhere have even been discovered Mycenæan lentoid gems. In Sicily the remarkable excavations of Prof. Orsi have brought to light a whole series of Mycenæan relics in the beehive rock-tombs of the south-eastern coast, associated with the later class of Sikel fabrics.

Sardinia, whose name has with great probability been connected with the Shardanas, who, with the Libyan and Ægean races, appear as the early invaders of Egypt, has already produced a Mycenæan gold ornament. An unregarded fact points further to the probability that it formed an important outpost of Mycenæan culture. In 1853 General Lamarmora first printed a MS. account of Sardinian antiquities, written in the later years of the fifteenth century by a certain Gilj, and accompanied by drawings made in 1497 by Johan Virde, of Sassari. Amongst these latter (which include, it must be said, some gross falsifications) is a capital and part of a shaft of a Mycenæan column in a style approaching that of the façade of the "Treasury of Atieus." It seems to have been found at a place near the Sardinian Olbia, and Virde has attached to it the almost prophetic description "*columna Pelasgica*." That it is not a fabrication due to some traveller from Greece is shown by a curious detail. Between the chevrons that adorn it are seen rows of eight-rayed stars, a detail unknown to the Mycenæan architectural decoration till it occurred on the painted base of the hearth in the *megaron* of the palace at Mycenæ excavated by the Greek Archæological Society in 1886. In this neglected record, then, we have an indication of the former existence in Sardinia of Mycenæan monuments, perhaps of palaces and royal tombs comparable to those of Mycenæ itself.

More isolated Mycenæan relics have been found still further afield, in Spain, and even the Auvergne, where Dr. Montelius has recognised an evidence of an old trade connection between the Rhone valley and the Eastern Mediterranean, in the occurrence of two bronze double axes of Ægean form. It is impossible to do more than indicate the influence exercised by the Mycenæan arts on those of the early Iron Age. Here it may be enough to cite the late Mycenæan parallels afforded by the Ægina Treasure to the open-work groups of bird-holding figures and the pendant ornaments of a whole series of characteristic ornaments of the Italo-Hallstatt culture.

In this connection, what may be called a sub-Mycenæan survival in the North-Western corner of the Balkan peninsula has a special interest for the Celtic West. Among the relics obtained by the fruitful excavations conducted by the Austrian archæologists in Bosnia and Herzegovina, and notably in the great prehistoric cemetery of Glasinatz, a whole series of Early Iron Age types betray distinct Mycenæan affinities. The spiral motive and its degeneration—the concentric circles grouped together with or without tangential lines of connection—appears on bronze torques, on fibulæ of Mycenæan descent, and the typical finger-rings with the besil at right angles to the ring. On the plates of other "spectacle fibulæ" are seen triquetral scrolls singularly recalling the gold plates of the Akropolis graves of Mycenæ. These, as well as other parallel survivals of the spiral system in the Late Bronze Age of the neighbouring Hungarian region, I have elsewhere[1] ventured to claim as the true source from which the Alpine Celts, together with many Italo-Illyric elements from the old Venetian province at the head of the Adriatic, drew the most salient features of their later style, known on the continent as that of La Tène. These Mycenæan survivals and Illyrian forms engrafted on the "Hallstatt" stock were ultimately spread by the conquering Belgic tribes to our own islands, to remain the root element of the Late Celtic style in Britain—where the older spiral system had long since died a natural death—and in Ireland to live on to supply the earliest decorative motives of its Christian art.

From a Twelfth Dynasty scarab to the book of Durrow or the font of Deerhurst is a far cry. But, as it was said of old, "Many things may happen in a long time." We have not to deal with direct transmission *per saltum*, but with gradual propagation through intervening media. This brief survey of "the Eastern Question in Anthropology" will not have been made in vain, if it helps to call attention to the mighty part played by the early Ægean culture as the mediator between primitive Europe and the older civilisations of Egypt and Babylonia. Adequate recognition of the Eastern background of the European origins is not the "Oriental Mirage." The independent European element is not affected by its power of assimilation. In the great days of Mycenæ we see it already as the equal, in many ways the superior, of its teachers, victoriously reacting on the older countries from which it had acquired so much. I may perhaps be pardoned if in these remarks, availing myself of personal investigations, I have laid some stress on the part which Crete has played in this first emancipation of the European genius. There far earlier than elsewhere we can trace

[1] Rhind Lectures, 1895, "On the Origins of Celtic Art," summaries of which appeared in the *Scotsman*.

the vestiges of primæval intercourse with the valley of the Nile. There more clearly than in any other area we can watch the continuous development of the germs which gave birth to the higher Ægean culture. There before the days of Phœnician contact a system of writing had already been worked out which the Semite only carried one step further. To Crete the earliest Greek tradition looks back as the home of divinely inspired legislation and the first centre of maritime dominion.

Inhabited since the days on the first Greek settlements by the same race, speaking the same language, and moved by the same independent impulses, Crete stands forth again to day as the champion of the European spirit against the yoke of Asia.

SECTION K.

BOTANY.

OPENING ADDRESS BY D. H. SCOTT, F.R.S., HONORARY KEEPER OF THE JODRELL LABORATORY, ROYAL GARDENS, KEW, PRESIDENT OF THE SECTION.

Present Position of Morphological Botany.

THE object of modern morphological botany (the branch of our science to which I propose to limit my remarks) is the accurate comparison of plants, both living and extinct, with the object of tracing their real relationships with one another, and thus of ultimately constructing a genealogical tree of the vegetable kingdom. The problem is thus a purely historical one, and is perfectly distinct from any of the questions with which physiology has to do.

Yet there is a close relation between these two branches of biology; at any rate, to those who maintain the Darwinian position. For from that point of view we see that all the characters which the morphologist has to compare are, or have been, adaptive. Hence it is impossible for the morphologist to ignore the functions of those organs of which he is studying the homologies. To those who accept the origin of species by variation and natural selection there are no such things as morphological characters pure and simple. There are not two distinct categories of characters—a morphological and a physiological category—for all characters alike are physiological. "According to that theory, every organ, every part, colour, and peculiarity of an organism must either be of benefit to an organism itself, or have been so to its ancestors. . . . Necessarily, according to the theory of natural selection, structures either are present because they are selected as useful, or because they are still inherited from ancestors to whom they were useful, though no longer useful to the existing representatives of those ancestors." (Lankester, "Advancement of Science," p. 307.)

The useful characters may have become fixed in comparatively recent times, or a long way back in the past. In the latter case the character in question may have become the property of a large group, and thus, as we say, may have become morphologically important.

For instance, parasitic characters, such as the suppression of chlorophyll, are equally adaptive in Dodder and in the Fungi. In Dodder, however, such characters are of recent origin and of little morphological importance, not hindering us from placing the genus in the natural order Convolvulaceæ; while in Fungi equally adaptive characters have become the common property of a great class of plants.

Then, again, the existence of a definite sporophyte generation, which is the great character of all the higher plants, is in certain Fungi inconstant, even among members of the same species.

Although there is no essential difference between adaptive and morphological characters, there is a great difference in the morphologist's and the physiologist's way of looking at them. The physiologist is interested in the question how organs work; the morphologist asks, what is their history?

The morphologist may well feel discouraged at the vastness of the work before him. The origin of the great groups of plants is perhaps, after all, an insoluble problem, for the question is not accessible either to observation or experiment.

All that we can directly observe or experiment upon is the occurrence of variations—perhaps the most important line of research in biology, for it was the study of variation that led Darwin and Wallace to their grand generalisation. Many observers are working to-day in the spirit of the great masters, and it is certain that their work will be fruitful in results. It is evident, however, that such investigations can at most only throw a side light on the historical question of the origin of the existing orders and classes of living things. The morphologist has to attack such questions by other methods of research.

The embryological method has so far scarcely received justice from botanists. A great deal of what is called embryology in botany is not embryology at all, but relates to pre-fertilisation changes. Of real embryology—that is to say, the development of the young plant from the fertilised ovum—there is much less than we might expect. Thus no comparative investigation of the embryology of either Dicotyledons or Monocotyledons has ever been carried out, our knowledge being entirely based on a few isolated examples.

In the cases which have been investigated perhaps excessive attention has been devoted to the first divisions of the ovum, the importance of which, as Sachs long ago showed, has been overrated, while the later stages, when the differentiation of organs and tissues is actually in progress, have been comparatively neglected.

The law of recapitulation (or repetition of phylogeny in ontogeny) has been very inadequately tested in the vegetable kingdom. Whatever its value may be, it is certainly desirable that the development of plants as well as animals should be considered from this point of view; and this has so far been done in but very few cases. M. Massart, of Brussels, has made some investigations with this object on the development of seedlings and of individual leaves. He is led to the conclusion that examples of recapitulation are rare among plants. ("La Récapitulation et l'Innovation en Embryologie Végétale," *Bull. de la Soc. roy. de Bot. de Belgique*, vol. xxxiii., 1894.)

So far, at least, embryological research has only yielded certain proof of recapitulation in a few cases, as in the well-known example of the phyllode-bearing acacias, in which the first leaves of the seedling are normal, while the later formed ones gradually assume the reduced phyllode form.

A less familiar example is afforded by *Gunnera*. Here, as is well known, the mature stem has a structure totally different from that of ordinary Dicotyledons, and much resembling that characteristic of most Ferns. In most species of *Gunnera* there are a number of distinct vascular cylinders in the stem, instead of one only, and there is never the slightest trace, so far as the adult plant is concerned, of the growth by means of cambium, which is otherwise so general in the class. The seedling stem, however, is not only monostelic below the cotyledons, but in this region, though nowhere else, shows distinct secondary growth. Thus, if we were in any doubt as to the general affinities of *Gunnera*, owing to its extraordinary mature structure, we should at once be put on the right track by the study of the embryonic stem, which alone retains the characterislic dicotyledonous mode of growth.

It is only in a few cases, however, and for narrow ranges of affinity, that the doctrine of recapitulation has at present helped in the determination of relationships among plants. Beyond this, conclusions based on embryology alone tend to become merely conjectural and subjective. In fact, all comparative work, in so far as it is limited to plants now living, suffers under the same weakness that it can never yield certain results, for the question whether given characters are relatively primitive or recently acquired is one upon which each naturalist is left to form his own opinion, as the origin of the characters cannot be observed.

To determine the blood-relationships of organisms it is necessary to decipher their past history, and the best evidence we can have (when we can get it) is from the ancient organisms themselves. The problem of the morphologist is an historical one, and contemporary documentary evidence is necessarily the best. It is palæontology alone which can give us the real historical facts.

ANATOMICAL CHARACTERS.

In judging of the affinities of fossil plants we are often compelled to make great use of vegetative characters, and more particularly of characters drawn from anatomical structure. It is true that in many cases we do so because we cannot help ourselves, such anatomical features being the only characters available in many of the specimens as at present known. But the value of the method has been amply proved in other cases where the reproductive structures have also been discovered, and are found to fully confirm the conclusions based on anatomy. I need only mention the great groups of the Lepidodendreæ and the Calamites, in each of which the anatomical characters, when accurately known, put us at once on the right track, and lead to

results which are only confirmed by the study of the reproductive organs.

In this matter fossil botany is likely to react in a beneficial way on the study of recent plants, calling attention to points of structure which have been passed over, and showing us the value of characters of a kind to which systematists had until recently paid but little attention. At present, owing to the work of Radlkofer, Vesque, and others, anatomical characters are gradually coming into use in the classification of the higher plants, and in some quarters there may even be a tendency to over-estimate their importance. Such exaggeration, however, is only a temporary fault incident to the introduction of a comparatively new method. In the long run nothing but good can result from the effort to place our classification on a broader basis. In most cases the employment of additional characters will doubtless serve only to further confirm the affinities already detected by the acumen of the older taxonomists. There are plenty of doubtful points, however, where new light is much needed; and even where the classification is not affected it will be a great scientific gain to know that its divisions are based on a comparison of the whole structure, and not merely on that of particular organs.

The fact that anatomical characters are adaptive is undeniable, but this applies to all characters, such difference as there is being merely one of degree. Cases are not wanting where the vegetative tissues show greater constancy than the organs of reproduction, as, for example, in the Marattiaceæ, where there is a great uniformity in anatomical structure throughout the family, while the sporangia show the important differences on which the distinction of the genera is based. It is in fact a mistake to suppose that anatomical characters are necessarily the expression of recent adaptations. On the contrary, it is easy to cite examples of marked anatomical peculiarities which have become the common property of large groups of plants.

For instance, to take a case in which I happen to have been specially interested, the presence of bast to the inside as well as to the outside of the woody zone is a modification of dicotyledonous structure which is in many groups, at least of ordinal value. The peculiarity is constant throughout the orders Onagraceæ, Lythraceæ, Myrtaceæ, Solanaceæ, Asclepiadaceæ, and Apocynaceæ, not to mention some less important groups. In other families, such as the Cucurbitaceæ and the Gentianeæ, it is nearly constant throughout the order, but subject to some exceptions. Among the Compositæ a similar, if not identical, peculiarity appears in some of the sub-order Cichoriaceæ, but is here not of more than generic value. In *Campanula* the systematic importance of internal phloëm is even less, for it appears in some species and not in others. Lastly, there are cases in which a similar character actually appears as an individual variation, as in *Carum Carvi*, and, under abnormal conditions, in *Phaseolus multiflorus*.

These latter cases seem to me worthy of special study, for in them we can trace, under our very eyes, the first rise of anatomical characters which have elsewhere become of high taxonomic importance. A comparative study of the anatomy of any group of British plants, taking the same species growing under different conditions, would be sure to yield interesting results if any one had the patience to undertake it.

Enough has been said to show that a given anatomical character may be of a high degree of constancy in one group while extremely variable in another, a fact which is already perfectly familiar as regards the ordinary morphological characters. For example, nothing is more important in phanerogamic classification than the arrangement of the floral organs as shown in ground-plan or floral diagram. Yet Prof. Trail's observations, which he has been good enough to communicate to me, show that in one and the same species, or even individual, of *Polygonum*, almost every conceivable variation of the floral diagram may be found.

There is, in fact, no "royal road" to the estimation of the relative importance of characters; the same character which is of the greatest value in one group may be trivial in another; and this holds good equally whether the character be drawn from the external morphology or from the internal structure.

Our knowledge of the comparative anatomy of plants, from this point of view, is still very backward, and it is quite possible that the introduction of such characters into the ordinary work of the Herbarium may be premature; certainly it must be conducted with the greatest judgment and caution. We have not yet got our data, but every encouragement should be given to the collection of such data, so that our classification in the future may rest on the broad foundation of a comparison of the entire structure of plants.

In estimating the relative importance of characters of different kinds we must not forget that characters are often most constant when most adaptive. Thus, as Prof. Trail informs me, the immense variability of the flowers of *Polygonum* goes together with their simple method of self-fertilisation. The exact arrangement is of little importance to the plant, and so variation goes on unchecked. In flowers with accurate adaptation to fertilisation by insects such variability is not found, for any change which would disturb the perfection of the mechanism is at once eliminated by natural selection.

Histology.

I propose to say but little on questions of minute histology, a subject which lies on the borderland between morphology and physiology, and which will be dealt with next Tuesday far more competently than I could hope to treat it. Last year my predecessor in the presidency of this Section spoke of a histological discovery (that of the nucleus, by Robert Brown) as "the most epoch-making of events" in the modern history of botany. The histological questions before us at the present day may be of no less importance, but we cannot as yet see them in proper perspective. The centrosomes, those mysterious protoplasmic particles which have been supposed to preside over the division of the nucleus, and thus to determine the plane of segmentation, if really permanent organs of the cell, would have to rank as co-equal with the nucleus itself. If, on the other hand, as some think, they are not constant morphological entities, but at most temporary structures differentiated *ad hoc*, then we are brought face to face with the question whether the causes of nuclear division lie in the nucleus itself or in the surrounding protoplasm.

Nothing can be more fascinating than such problems, and nothing more difficult. We have, at any rate, reason to congratulate ourselves that English botanists are no longer neglecting the study of the nucleus and its relation to the cell. For a long time little was done in these subjects in our country, or at least little was published, and botanists were generally content to take their information from abroad, not going beyond a mere verification of other men's results. Now we have changed all that, as the communications to this Section sufficiently testify.

Nothing is more remarkable in histology than the detailed agreement in the structure and behaviour of the nucleus in the higher plants and the higher animals, an agreement which is conspicuously manifest in those special divisions which take place during the maturation of the sexual cells. Is this striking agreement the product of inheritance from common ancestors, or is the parallelism dependent solely on similar physical conditions in the cells? This is one of the great questions upon which we may hope for new light from the histological discussion next week.

Alternation of Generations.

We have known ever since the great discoveries of Hofmeister that the development of a large part of the vegetable kingdom involves a regular alternation of two distinct generations, the one, which is sexual, being constantly succeeded—so far as the normal cycle is concerned—by the other which is asexual. This alternation is most marked in the mosses and ferns, taking these words in their widest sense, as used by Prof. Campbell in his recent excellent book. In the Bryophyta, the ordinary moss or liverwort plant is the sexual generation, producing the ovum, which, when fertilised, gives rise to the moss-fruit, which here alone represents the asexual stage. The latter forms spores from which the sexual plant is again developed.

In the Pteridophyta the alternation is equally regular, but the relative development of the two generations is totally different, the sexual form being the insignificant prothallus, while the whole fern-plant, as we ordinarily know it, is the asexual generation.

The thallus of some of the lower Bryophyta is quite comparable with the prothallus of a fern, so as regards the sexual generation there is no difficulty in seeing the relation of the two classes; but when we come to the asexual generation or sporophyte the case is totally different. There is no appreciable resemblance between the fruit of any of the Bryophyta and the plant of any vascular Cryptogam.

There is thus a great gap within the Archegoniatæ; there is

another at the base of the series, for the regular alternation of the Bryophyta is missing in the Algæ and Fungi, and the question as to what corresponds among these lower groups to the sporophyte and oöphyte of the higher Cryptogams is still disputed.

Now as regards this life-cycle, which is characteristic of all plants higher than Algæ and Fungi, there are two great questions at present open. The one is general: are the two generations, the sporophyte and the oöphyte, homologous with one another, or is the sporophyte a new formation intercalated in the life-history, and not comparable to the sexual plant? The former kind of alternation has been called homologous, the latter antithetic. This question involves the *origin* of alternation; its solution would help us to bridge over the gap between the Archegoniatæ and the lower plants. The second problem is more special: has the sporophyte of the Pteridophyta, which always appears as a complete plant, been derived from the simple and totally different sporophyte of the Bryophyta, or are the two of distinct origin?

At present it is usual, at any rate in England, to assume the antithetic theory of alternation. Prof. Bower, its chief exponent, says ("Spore-producing Members," *Phil. Trans.*, vol. clxxxv. B. (1894), p. 473): "It will also be assumed that, whatever may have been the circumstances which led to it, antithetic alternation was brought about by elaboration of the zygote [*i.e.* the fertilised ovum] so as to form a new generation (the sporophyte) interpolated between successive gametophytes, and that the neutral generation is not in any sense the result of modification or metamorphosis of the sexual, but a new product having a distinct phylogenetic history of its own." In his essay on "Antithetic as distinguished from Homologous Alternation of Generations in Plants" ("Annals of Botany," vol. iv. (1890) p. 362), the author describes the hypothetical first appearance of the sporophyte as follows: "Once fertilised, a zygote might in these plants [the first land plants] divide up into a number of portions (carpospores), each of which would then serve as a starting-point of a new individual."

On this view, the sporophyte first appeared as a mere group of spores formed by the division of the fertilised ovum. Consequently the inference is drawn that all the vegetative parts of the sporophyte have arisen by the "sterilisation of potentially sporogenous tissue." That is to say, there was nothing but a mass of spores to start with, so whatever other tissues and organs the sporophyte may form must be derived from the conversion of spore-forming cells into vegetative cells. Prof. Bower has worked out this view most thoroughly, and as the result he is not only giving us the most complete account of the development of sporangia which we have ever had, but he has also done much to clear up our ideas, and to show us what the course of evolution ought to have been if the assumptions required by the antithetic theory were justified.

Without entering into any detailed criticism of this important contribution to morphology, which is still in progress, I wish to point out that we are not, after all, bound to accept the assumption on which the theory rests. There is another view in the field, for which, in my opinion, much is to be said. The antithetic theory is receiving a most severe test at the friendly hands of its chief advocate. Should it break down under the strain we need not despair, for another hypothesis remains which I think quite equally worthy of verification.

This is the theory of Pringsheim, according to which the two generations are *homologous* one with another, the oöphyte corresponding to a sexual individual among Thallophytes, the sporophyte to an asexual individual. To quote Pringsheim's own words ("Gesammelte Abhandlungen," II. p. 370): "The alternation of generations in mosses is immediately related to those phenomena of the succession of free generations in Thallophytes, of which the one represents the neutral, the other the sexual plant." Further on (*ibid.*, p. 371) he illustrates this by saying: "The moss sporogonium stands in about the same relation to the moss plant as the sporangium-bearing specimens of *Saprolegnia* stand to those which bear oögonia, or as, among the Florideæ, the specimens with tetraspores are related to those with cystocarps." This gets rid of the intercalation of a new generation altogether; we only require the modification of the already existing sexual and asexual forms of the Thallophytes.

The sudden appearance of something completely new in the life-history, as required by the antithetic theory, has, to my mind, a certain improbability. *Ex nihilo nihil fit.* We are not accustomed in natural history to see brand-new structures appearing, like morphological Melchizedeks, without father or mother. Nature is conservative, and when a new organ is to be formed it is, as every one knows, almost always fashioned out of some pre-existing organ. Hence I feel a certain difficulty in accepting the doctrine of the appearance of an intercalated sporophyte by a kind of special creation.

We can have no direct knowledge of the origin of the sporophyte in the Bryophyta themselves, for the stages, whatever they may have been, are hopelessly lost. In some of the Algæ, however, we find what most botanists recognise as at least a parallel development, even if not phylogenetically identical. (See Bower, "Antithetic Alternation," p. 361). In *Œdogonium*, for example, the oöspore does not at once germinate into a new plant, but divides up into four active zoospores, which swim about and then germinate. In *Coleochæte* the oöspore actually becomes partitioned up by cell-walls into a little mass of tissue, each cell of which then gives rise to a zoospore.

In both these genera (and many more might be added) the cell-formation in the germinating oöspore has been generally regarded as representing the formation of a rudimentary sporophyte generation. If we are to apply the antithetic theory of alternation to these cases, we must assume that the zoospores produced on germination are a new formation, intercalated at this point of the life-cycle. But is this assumption borne out by the facts? I think not. In reality nothing new is intercalated at all. The "zoospores" formed from the oöspore on germination are identical with the so-called "zoogonidia," formed on the ordinary vegetative plant at all stages of its growth.

In science, as in every subject, we too easily become the slaves of language. By giving things different names we do not prove that the things themselves are different. In this case, for example, the multiplication of terms serves, in my opinion, merely to disguise the facts. The reproductive cells produced by the ordinary plant of an *Œdogonium* are identical in development, structure, behaviour, and germination with those produced by the oöspore. The term "zoogonidia" applied to the former is a "question-begging epithet," for it assumes that they are not homologous with the "zoospores" produced by the latter. I prefer to keep the old name zoospore for both, as they are identical bodies.

To my mind the point seems to be this. An *Œdogonium* (to keep to this example) can form zoospores at any stage of its development; there is one particular stage, however, at which they are *always* formed—namely, on the germination of the oöspore. Nothing new is intercalated, but the irregular and indefinite succession of sexual and asexual acts of reproduction is here tending to become regular and definite.

In *Sphæroplea*, as was well pointed out by the late Mr. Vaizey (*Annals of Botany*, vol. iv. p. 373), though his view of alternation was very different from that which I am now putting forward, the alternation is as definite as in a moss, for here, so far as we know, zoospores are only formed on the germination of the fertilised ovum. If *Sphæroplea* stood alone we might believe in the intercalation of these zoospores, as a new stage, but the comparison with *Ulothrix*, *Œdogonium*, *Bulbochæte* and *Coleochæte* shows, I think, where they came from.

The body formed from the oöspore is called by Pringshei the first neutral generation. In *Œdogonium* this has n vegetative development, for the first thing that the oöspore does is to form the asexual zoospores, and it is completely used up in the process. In other cases it is not in quite such a hurry, and here the first neutral generation has time to show itself as an actual plant. This is so in *Ulothrix*, a much more primitive form than *Œdogonium*, for its sexuality is not yet completely fixed. Here the zygospore actually germinates, forming a dwarf plant, and in this stage passes through the dull season, producing zoospores when the weather becomes more favourable. On Pringsheim's view the dwarf plant is not a new creation, but just a rudimentary *Ulothrix*, which soon passes on to spore-formation. So, too, with the cellular body formed on the germination of the oöspore of *Coleochæte;* this also is looked upon as a reduced form of thallus. On any view this genus is especially interesting, for the sporophyte remains enclosed by the tissue of the sexual generation, thus offering a striking analogy with the Bryophyta.

In the Phycomycetous Fungi—plants which have lost their chlorophyll, but which otherwise in many cases scarcely differ from Algæ—the oöspore in one and the same species may either form a normal mycelium, or a rudimentary mycelium

bearing a sporangium, or may itself turn at once into a sporangium (producing zoospores) without any vegetative development. Here it seems certain that Pringsheim's view is the right one, for all stages in the reduction of the first neutral generation lie before our eyes. Nowhere, either here or among the green Algæ, do I see any evidence for the intercalation of a *new* generation or a new form of spore on the germination of the fertilised ovum.

Pringsheim extends the same view to the higher plants. The sporogonium of a moss is for him the highly modified first neutral generation, homologous with the vegetative plant, but here specially adapted for spore-formation. I have elsewhere pointed out (NATURE, February 21, 1895) that this view has great advantages, for not only does it harmonise exactly with the actual facts observed in the green Algæ and their allies, but it also helps us to understand the astoundingly different forms which the archegoniate sporophyte may assume.

It seems to me that Pringsheim was right in regarding the fruit-formation of Floridæ as totally different from the sporophyte-formation of *Coleochæte* or the Bryophyta. The cystocarp bears none of the marks of a distinct generation, for throughout its whole development it remains in the most complete organic connection with the thallus that bears it. The whole Floridean process, often so complicated, appears to be an arrangement for effecting the fertilisation of many female cells as the result of an original impregnation by a single sperm-cell. There is here still a great field for future research; but in the light of our present knowledge there seems to be no real parallelism with the formation of a sporophyte in the higher plants.

The gap between the Bryophyta and the Algæ remains, unfortunately, a wide and deep one, and it is not probable that any Algæ at present known to us lie at all near the line of descent of the higher Cryptogams. *Riccia* is often compared with *Coleochæte*, but it is by no means evident that *Riccia* is a specially primitive form. In *Anthoceros*, which bears some marks of an archaic character, the sporophyte is relatively well developed. To those who do not accept the theory of intercalation it is not necessary to assume that the most primitive Bryophyta must have the most rudimentary sporophyte.

Apart from other differences, Bryophyta differ from most green Algæ in the fact that asexual spores are *only found* in the generation succeeding fertilisation. The spores moreover are themselves quite different from anything in Algæ, and the constancy of their formation in fours among all the higher plants from the liverworts upwards, is a fact which requires explanation. I should like to suggest to some energetic histologist a comparison of the details of spore-formation in the lower liverworts and in the various groups of Algæ, especially those of the green series. It is possible that some light might be thus thrown on the origin of tetrad-spore-formation, a subject as to which Prof. Farmer has already gained some very remarkable results. On Pringsheim's view some indications or homology between bryophytic and algal spore-formation might be expected, and anyhow the tetrads require *some* explanation.

The peculiarities of the sporophyte in the Archegoniatæ, as compared with any algal structures, depend, no doubt, on the acquirement of a terrestrial habit, while the oöphyte by its mode of fertilisation remains "tied down to a semi-aquatic life." (Bower, "Antithetic Alternation.") Prof. Bower's phrase "amphibious alternation" expresses this view of the case very happily, and indeed his whole account of the rise of the sporophyte is of the highest value, even though we may not accept his assumption as to its origin *de novo*.

I attach special weight to Prof. Bower's treatment of this subject, because he has shown how the most important of all morphological phenomena in plants, namely the alternation of generations in Archegoniatæ, may be explained as purely adaptive in origin. All Darwinians owe him a debt of gratitude for this demonstration, which holds good even if we believe the sporophyte to be the modification of a pre-existing body, and not a new formation.

APOSPORY AND APOGAMY.

We must remember that the theory of homologous alternation has twice received the strongest confirmation of which a scientific hypothesis is susceptible—that of verified prediction. In both cases Pringsheim was the happy prophet. Convinced on structural grounds of the homology of the two generations in mosses, he undertook his experiments on the moss-fruits, in the hope, as he says ("Ges. Abh." II., p. 407), that he would succeed in producing protonema from the subdivided seta of the mosses, and thus prove the *morphological* agreement of seta and moss-stem. His experiment, as everybody knows, was completely successful, and resulted in the first observed cases of *apospory*, *i.e.* the direct outgrowth of the sexual from the asexual generation.

Here he furnished his own verification; in the second case it has come from other hands. In the paper of 1877, so often referred to, he says (p. 391): "Here, however [*i.e.* in the ferns], the act of generation, that is, the formation of sexual organs and the origin of an embryo, is undoubtedly bound up with the existence of the spore, *until those future ferns are found* which I indicated as conceivable in my preliminary notice, *in which the prothallus will sprout forth directly from the frond.*"

It is unnecessary to remind English botanists that Pringsheim's hypothetical aposporous ferns are now perfectly well known in the flesh; such cases having been first observed by Mr. Druery and then fully investigated by Prof. Bower.

A very remarkable case of direct origin of the oophyte from the sporophyte has lately been described by Mr. E. J. Lowe, in a variety of *Scolopendrium vulgare*. Here the young fern-plant produced prothalli bearing archegonia as direct outgrowths from its second or third frond. The specimen had a remarkable history, for the young plants were produced from portions of a prothallus which had been kept alive and repeatedly subdivided during a period of no less than eight years. I cannot go into the interesting details here, they will be published elsewhere; but I wish to call attention to the fact that in this case the production of the sexual from the asexual generation, occurring so early in life, has no obvious relation to suppressed spore-formation, and so appears to differ essentially from the cases first described, which occurred on mature plants. I believe Mr. Lowe's case is not an altogether isolated one.

The converse phenomenon—that of apogamy—or the direct origin of an asexual plant from the prothallus without the intervention of sexual organs, has now been observed in a considerable number of ferns, the examples already known belonging to no less than four distinct families: Polypodiaceæ, Parkeriaceæ, Osmundaceæ, and Hymenophyllaceæ. In *Trichomanes alatum* Prof. Bower found that apospory and apogamy co-exist in the same plant, the sporophyte directly giving rise to a prothallus, which again directly grows out into a sporophyte; the life-cycle is thus completed without the aid either of spores or of sexual organs. Dr. W. H. Lang, who has recently made many interesting observations on apogamy, will, I am glad to say, read a paper on the subject before this Section, so I need say no more.

I must, however, express my own conviction that the facility with which, in ferns, the one generation may pass over into the other by vegetative growth, and that in both directions, is a most significant fact. It shows that there is no such hard and fast distinction between the generations as the antithetic theory would appear to demand, and in my opinion weighs heavily on the side of the homology of sporophyte and oöphyte. I cannot but think that the phenomena deserve greater attention from this point of view than they have yet received.

A mode of growth which affords a perfectly efficient means of abundant propagation cannot, I think, be dismissed as merely teratological.

Since the foregoing paragraph was first written Dr. Lang has made the remarkable discovery (already communicated to the Royal Society) that in a *Lastræa* sporangia of normal structure are produced on the prothallus itself, side by side with normal archegonia and antheridia. I cannot forbear mentioning this striking observation, of which we shall hear an account from the discoverer himself.

The strongest advocate of the homology of the prothallus with the fern plant could scarcely have ventured to anticipate such a discovery.

RELATION BETWEEN MOSSES AND FERNS.

Goebel said, in 1882: "The gap between the Bryophyta and the Pteridophyta is the deepest known to us in the vegetable kingdom. We must seek the starting-point of the Pteridophyta elsewhere than among the Muscinæ: among forms which may have been similar to liverworts, but in which the asexual generations entered from the first on a different course of development." (Schenk's "Handbuch der Botanik," vol. ii. p. 401.) I cannot help feeling that all the work which has been done since goes to confirm this wise conclusion. Attempts have been made in the most sportsmanlike manner (to adopt a phrase of Prof. Bower's)

to effect a passage over the gulf, but the gulf is still unbridged. I cannot see anywhere the slightest indication of anything like an intermediate form between the spore-bearing plant of the Pteridophyta and the spore-bearing fruit of the Bryophyta. The plant of the Pteridophyta is sometimes small and simple, but the smallest and simplest seem just as unlike a bryophytic sporogonium as the largest and most complex. On the side of the moss group, *Anthoceros* has been often cited as a form showing a certain approach towards the Pteridophytes, and Prof. Campbell in particular has developed this idea with remarkable ingenuity. An unprejudiced comparison, however, seems to me to show nothing more here than a very remote parallelism, not suggestive of affinity.

There is no reason to believe that the Bryophyta, as we know them, were the precursors of the vascular Cryptogams at all. There is a remarkable paucity of evidence for the geological antiquity of Bryophyta, though man of the mosses at any rate would seem likely to have been preserved if they existed. Brongniart said, in 1849, "The rarity of fossil mosses, and their complete absence up to now in the ancient strata, are among the most singular facts in geological botany" ("Tableau des Genres de Végétaux Fossiles, p. 13); and since that time it is wonderful how little has been added. Things seem to point to both Pteridophyta and Bryophyta having had their origin far back among some unknown tribes of the Algæ. If we accept the homologous theory of alternation, we may fairly suppose that the sporophyte of the earliest Pteridophyta always possessed vegetative organs of some kind. The resemblance between the young sporophyte and the prothallus in some lycopods indicates that at some remote period the two generations may not have been very dissimilar. At least some such idea gives more satisfaction to my mind than the attempt to conceive of a fern-plant as derived from a sterilised group of potential spores.

The Bryophyta may have had from the first a more reduced sporophyte, the first neutral generation having, in their ancestors, become more exclusively adapted to spore-producing functions. I must not omit to mention the idea that the Bryophyta, or at any rate the true mosses, are degenerate descendants of higher forms. The presence of typical stomata on the capsule in some cases, and of somewhat reduced stomata in others, has been urged in support of this view. It is possible; but if so, from what have these plants been reduced?

Few people, perhaps, fully realise how absolutely insoluble such a problem as we have been discussing really is. I say nothing as to the mosses, which *may* have arisen relatively late in geological history. The Pteridophyta, at any rate, are known to be of inconceivable antiquity. Not only did they exist in greater development than at present in the far-off Devonian period, but at that time they were already accompanied by highly organised gymnospermous flowering-plants. Probably we are all agreed that Gymnosperms arose somehow from the vascular Cryptogams. Hence, in the Devonian epoch, there had already been time not only for the Pteridophyta themselves to attain their full development, but for certain among them to become modified into complex Phanerogams. It would not be a rash assumption that the origin of the Pteridophyta took place as long before the period represented by the plant-bearing Devonian strata as that period is before our own day. Can we hope that a mystery buried so far back in the dumb past will be revealed.

It will be understood that I do not wish to assume the *rôle* of partisan for the homologous theory of alternation. Possibly the whole question lies beyond human ken, and partisanship would be ridiculous. But I do wish to raise a protest against anything like a dogmatic statement that alternation of generations *must have been* the result of the interpolation of a new stage in the life-history. Let us, in the presence of the greatest mystery in the morphology of plants, at least keep an open mind, and not tie ourselves down to assumptions, though we may use them as working hypotheses.

HISTOLOGICAL CHARACTERS OF THE TWO GENERATIONS.

There is one histological question upon which I must briefly touch because it bears directly on the subject which we have been considering. I shall say very little, however, in view of the forthcoming discussion.

It is now well known that in animals and in the higher plants a remarkable numerical change takes place in the constituents of the nucleus shortly before the act of fertilisation. The change consists in the halving of the number of chromosomes, those rod-like bodies which form the essential part of the nucleus, and are regarded by Weismann and most biologists as the bearers of hereditary qualities. Thus in the lily the number of chromosomes in the nuclei of vegetative cells is twenty-four; in the sexual nuclei, those of the male generative cell and of the ovum, the number is twelve. When the sexual act is accomplished the two nuclei unite, and so the full number is restored and persists throughout the vegetative life of the next generation. The absolute figures are of course of no importance; the point is, the reduction to one half during the maturation of the sexual cells, and the subsequent restoration of the full number when their union takes place. I say nothing as to the details or the significance of the process, points which have been fully dealt with elsewhere, notably in an elaborate recent paper by Miss E. Sargant.

Now, in animals (so far as I am aware) and in angiospermous plants the reduction of the chromosomes takes place very shortly before the differentiation of the sexual cells. Thus in a lily the reduction takes place on the male side immediately prior to the first division of the pollen mother-cell, so that four cell-divisions in all intervene between the reduction and the final differentiation of the male generative cells. On the female side the reduction in the same plant takes place in the primary nucleus of the embryo-sac, so that here there are three divisions between the reduction and the formation of the ovum. I believe these facts agree very closely with those observed in the animal kingdom, and so far there is no particular difficulty, for we can easily understand that if the number of chromosomes is to be kept constant from one generation to another, then the doubling involved in sexual fusion must necessarily be balanced by a halving.

There are, however, a certain number of observations on Gymnosperms and archegoniate Cryptogams which appear to put the matter in a different light. Overton ("Annals of Botany," vol. vii. p. 139), first showed that in a Cycad, *Ceratozamia*, the nuclei of the prothallus or endosperm all have the half-number of chromosomes. Here then the reduction takes place in the embryo sac (or rather its mother-cell), but a great number of cell-generations intervene between the reduction and the maturation of the ovum. In fact the whole female oöphyte shows the reduced number, while the sporophyte has the full number. The reduction takes place also in the pollen mother-cell. Further observations have extended this conclusion to some other Gymnosperms.

In *Osmunda* among the ferns there is evidence to show that reduction takes place in the spore mother-cell, and that the sexual generation has the half-number throughout. Prof. Farmer has found the same thing in various liverworts, and shown that the reduction of chromosomes takes place in the spore mother-cell; and his observations of cell-division in the two generations have afforded some direct evidence that the oöphyte has the half-number and the sporophyte the full number throughout. Prof. Strasburger fully discussed this subject before Section D at Oxford (see "Annals of Botany," vol. viii. p. 281), and came to the conclusion that the difference in number of chromosomes is a difference between the two generations as such, the sexual generation being characterised by the half-number, the asexual by the full number.

The importance of this conception for the morphologist is that an actual histological difference appears to be established between the two generations; a fact which would appear to militate against their homology. Some botanists even go so far as to propose making the number of chromosomes the criterion by which the two generations are to be distinguished. Considering that the whole theory rests at present on but few observations, I venture to think this both premature and objectionable; for nothing can be worse for the true progress of science than to rush hastily to deductive reasoning from imperfectly established premises.

The facts are certainly very difficult to interpret. Those who accept the antithetic theory of alternation suppose the sexual generation to be the older, and that in Thallophytes the plant is always an oöphyte, whether "actual" or "potential." Hence they believe that in Thallophytes the plant should show throughout the reduced number of chromosomes, reduction hypothetically taking place immediately upon the germination of the oöspore. If this were true it would lend some support to the idea of the intercalation of the sporophyte, but at present there is not the slightest evidence for these assumptions. On the contrary, in the only Thallophyte in which chromosome-counting

has been successfully accomplished (*Fucus*), Prof. Farmer and Mr. Williams find exactly the reverse; the plant has throughout the *full* number of chromosomes; reduction first takes place in the oögonium, immediately before the maturation of the ova, and on sexual fusion the full number is restored, to persist throughout the vegetative life of the plant. *Fucus* is, no doubt, a long way off the direct line of descent of Archegoniatæ, but still it is a striking fact that the only direct evidence we have goes dead against the idea that the sexual generation (and who could call a Fucus-plant anything else but sexual?) necessarily has the reduced number of chromosomes. This fact is indeed a rude rebuff to deductive morphology.

I am disposed to regard the different number of chromosomes in the two generations observed in certain cases among Archegoniatæ not as a primitive but as an acquired phenomenon, perhaps correlated with the definiteness of alternation in the Archegoniatæ as contrasted with its indefiniteness in Thallophytes. In *Fucus*, in flowering plants, and in animals the *soma* or vegetative body has the full number of chromosomes. With these the sporophyte of the Archegoniatæ agrees; it is the oöphyte which appears to be peculiar in possessing the half-number, so that if the evidence points to intercalation at all, it would seem to suggest that the oöphyte is the intercalated generation—obviously a *reductio ad absurdum*. I do not think we are as yet in a position to draw any morphological conclusions from these minute histological differences, interesting as they are.

The question how the number of chromosomes is kept right in cases of apospory and of apogamy is obviously one of great interest, and I am glad to say that it is receiving attention from competent observers.

Sexuality of Fungi.

Only a few years ago De Bary's opinion that the fruit of the ascus-bearing Fungi is normally the result of an act of fertilisation was almost universally accepted, especially in this country. Although the presence of sexual organs had only been recorded in comparatively few cases, and the evidence for their functional activity was even more limited, yet the conviction prevailed that the ascocarp is at least the homologue of a sexually produced fruit. The organ giving rise to the ascus or asci was looked upon as homologous with the oögonium of the Peronosporeæ, the supposed fertilising organ either taking the form of an antheridial branch as in that group, or, as observed by Stahl in the lichen *Collema*, giving rise to distinct male cells, or spermatia. More recently there has been a complete revolution of opinion on this point, and a year ago or less most botanists probably agreed that the question of the sexuality of the Ascomycetes had been settled in a negative sense. This change was due, in the first place, to the influence of Brefeld, who showed, in a great number of laborious investigations, that the ascus-fruit may develop without the presence of anything like sexual organs; while Möller proved that the supposed male cells of lichens are in a multitude of cases nothing but conidia, capable of independent germination.

The view thus gained ground that all the higher Fungi are asexual plants, fertilisation only occurring in the lower forms, such as the Peronosporeæ and Mucorineæ, which have not diverged far from the algal stock. The ascus, in particular, is regarded by this school as homologous with the asexual sporangium of a *Mucor*. This theory has been brilliantly expounded in a remarkable book by Von Tavel, which we cannot but admire as a model of clear morphological reasoning, whether its conclusions be ultimately adopted or not.

Still, it must be admitted that the Brefeld school were rather apt to ignore such pieces of evidence as militated against their views, and consequently their position was insecure so long as those hostile posts were left uncaptured.

Quite recently the whole question has been reopened by the striking observations of Mr. Harper, an American botanist working at Bonn.

Zopf, in 1890 (Die Pilze, "Schenk's Handbuch der Botanik," Bd. iv. p. 341), pointed out that up to that time it had not been possible in any Ascomycete to demonstrate a true process of fertilisation by strictly scientific evidence, namely, by observing the fusion of the nuclei of the male and female elements. Exactly the proof demanded has now been afforded by Mr. Harper's observations, for in a simple Ascomycete, *Sphærotheca castagnei*, the parasite causing the hop-mildew, he has demonstrated in a manner which appears to be conclusive the fusion of the nucleus of the antheridium with that of the ascogonium (*Berichte der deutschen bot. Gesellschaft*, vol. xiii., January 29, 1896). It is impossible to evade the force of this evidence, for the fungus in question is a perfectly typical Ascomycete, though exceptionally simple, in so far as only a single ascus is normally produced from the ascogonium. It is unnecessary to point out how important it is that Mr. Harper's observations should be confirmed and extended to other and more complex members of the order. In the mean time the few who (unlike your President) had not bowed the knee to Brefeld may rejoice!

It is impossible to pursue the various questions which press upon one's mind in considering the morphology of the Fungi. The occurrence not only of cell-fusion, but of nuclear fusion, apart from any definite sexual process, now recorded in several groups of Fungi, urgently demands further inquiry. Such unions of nuclei have been observed in the basidia of Agarics, the teleutospores of Uredineæ, and even in the asci of the Ascomycetes. That such a fusion is not necessarily, as Dangeard (*Le Botaniste*, vols. iv. and v.) has supposed, of a sexual nature, seems to be proved by the fact that it occurs in the young ascus of *Sphærotheca* long after the true act of fertilisation has been accomplished. It is possible, however, that these phenomena may throw an important side-light on the significance of the sexual act itself.

Another question which is obviously opened up by the new results is that of the homologies of the ascus. The observations of Lagerheim ("Pringsheim's Jahrbuch f. Wiss Bot.," 1892), on *Dipodascus* point to the sexual origin of a many-spored sporangium not definitely characterised as an ascus. On the other hand, not only sporangia, but true asci are known to arise in a multitude of cases direct from the mycelium. It is of course possible that as regards the asci these are cases of reduction or apogamy; on the other hand, it is not wholly impossible that the asci may turn out to be really homologous with a sexual sporangia, even though their development may often have become associated with the occurrence of a sexual act. However this may be, there is at present no reason to doubt that a very large proportion of the Fungi are, at least functionally, sexless plants.

Chalazogamy.

Among the most striking results of recent years bearing on the morphology of the higher plants, Treub's discovery of the structure of the ovule and the mode of fertilisation in *Casuarina* must undoubtedly be reckoned. The fact that the pollen tube in this genus does not enter the micropyle, but travels through the tissues of the ovary to the chalaza, thus reaching the base of the embryo-sac, was remarkable enough in itself, and when considered in connection with the presence of a large sporogenous tissue producing numerous embryo-sacs, appeared to justify the separation of this order from other angiosperms. Then came the work of Miss Benson in England, and of Nawaschin in Russia, showing that these remarkable peculiarities are by no means confined to *Casuarina*, but extend also in various modifications to several genera of the Cupuliferæ and Ulmaceæ. They are not, however, constant throughout these families, so that we are no longer able to attach to these characters the same fundamental systematic importance which their first discoverer attributed to them. It is remarkable, however, that these departures from the ordinary course of angiospermous development occur in families some of which have been believed on other grounds to be among the most primitive Dicotyledons.

Evidence of Descent derived from Fossil Botany.

At the beginning of this Address I spoke of the importance of the comparatively direct evidence afforded by fossil remains as to the past history of plants. It may be of interest if I endeavour to indicate the directions in which such evidence seems at present to point.

It was Brongniart who in 1828 first arrived at the great generalisation that "nearly all of the plants living at the most ancient geological epochs were Cryptogams" (Williamson, "Reminiscences of a Yorkshire Naturalist," 1896, p. 198), a discovery of unsurpassed importance for the theory of evolution, though one which is now so familiar that we almost take it for granted. Those palæozoic plants which are not Cryptogams are Gymnosperms, for the angiospermous flowering plants only make their appearance high up in the secondary rocks. Even

the Wealden flora, recently so carefully described by Mr. Seward, one of the secretaries to this Section, has as yet yielded no remains referable to Angiosperms, though this is about the horizon at which we may expect their earliest trace to be found.

Attention has already been called to the enormous antiquity of the higher Cryptogams—the Pteridophyta—and to the striking fact that they are accompanied, in the earliest strata in which they have been demonstrated with certainty, by well-characterised Gymnosperms. The Devonian flora, so far as we know it, though an early, was by no means a primitive one, and the same statement applies still more strongly to the plants of the succeeding Carboniferous epoch. The palæozoic Cryptogams, as is now well known, being the dominant plants of their time, were in many ways far more highly developed than those of our own age; and this is true of all the three existing stocks of Pteridophyta, Ferns, Lycopods, and Equisetineæ.

We cannot therefore expect any *direct* evidence as to the origin of these groups from the palæozoic remains at present known to us, though it is, of course, quite possible that the plants in question have sometimes retained certain primitive characters, while reaching in other respects a high development. For example, the general type of anatomical structure in the young stems of the Lepidodendreæ was simpler than that of most Lycopods at the present day, though in the older trunks the secondary growth, correlated with arborescent habit, produced a high degree of complexity. On the whole, however, the interest of the palæozoic Cryptogams does not consist in the revelation of their primitive ancestral forms, but rather in their enabling us to trace certain lines of evolution further upward than in recent plants. From the Carboniferous rocks we first learn what Cryptogams are capable of. In descending to the early strata we do not necessarily trace the trunk of the genealogical tree to its base; on the contrary, we often light on the ultimate twigs of extensive branches which died out long before our own period.

In a lecture which I had the honour of giving last May before the Liverpool Biological Society, I pointed out how futile the search for "missing links" among fossil plants is likely to be. The lines of descent must have been so infinitely complex in their ramification that the chances are almost hopelessly great against our happening upon the direct ancestors of living forms. Among the collateral lines, however, we may find invaluable indications of the course of descent.

Fossil botany has revealed to us the existence in the Carboniferous epoch of a fourth phylum of vascular Cryptogams quite distinct from the three which have come down—more or less reduced—to our own day. This is the group of Sphenophylleæ, plants with slender ribbed stems, superposed whorls of more or less wedge-shaped leaves, and very complex strobili with stalked sporangia. The group to a certain extent combines the characters of Lycopods and Horsetails, resembling the former in the primary anatomy, and the latter, though remotely, in external habit and fructification. Like so many of the early Cryptogams, *Sphenophyllum* possessed well-marked cambial growth. One may hazard the guess that this interesting group may have been derived from some unknown form lying at the root of both Calamites and Lycopods. The existence of the Sphenophylleæ certainly suggests the probability of a common origin for these two series.

In few respects is the progress made recently in fossil botany more marked than in our knowledge of the affinities of the Calamarieæ. Even so recently as the publication of Count Solms-Laubach's unrivalled introduction to "Fossil Botany," the relation of this family to the Horsetails was still so doubtful that the author dealt with the two groups in quite different parts of his book. This is never likely to happen again. The study of vegetative anatomy and morphology on the one hand, and of the perfectly preserved fructifications on the other, can leave no doubt that the fossil Calamarieæ and the recent Equiseta belong to one and the same great family, of which the palæozoic representatives are, generally speaking, by far the more highly organised. This is not only true of their anatomy, which is characterised by secondary growth in thickness just like that of a Gymnosperm, but also applies to the reproductive organs, some of which are distinctly heterosporous. In the genus *Calamostachys* we are, I think, able to trace the first rise of this phenomenon.

The external morphology of the cones is also more varied and usually more complex than that of recent Equiseta, though in some Carboniferous forms, as in the so-called *Calamostachys tenuissima* of Grand' Eury, we find an exactly Equisetum-like arrangement.

The position of the Sigillariæ as true members of the Lycopod group, is now well established. The work of Williamson proved that there is no fundamental distinction between the vegetative structure of *Lepidodendron*, which has always been recognised as lycopodiaceous, and that of *Sigillaria*. Secondary growth in thickness, the character which here, as in the case of the Calamodendreæ, misled Brongniart, is the common property of both genera. Then came Zeiller's discovery of the cones of *Sigillaria*, settling beyond a doubt that they are heterosporous Cryptogams. A great deal still remains to be done, more especially as to the relation of *Stigmaria* to the various types of lycopodiaceous stem. At present we are perhaps too facile in accepting *Stigmaria ficoides* as representing the underground organs of almost any carboniferous Lycopod.

We are now in possession of a magnificent mass of data for the morphology of the palæozoic lycopods, and have, perhaps, hardly yet realised the richness of our material. I refer more especially to specimens with structure, on which, here as elsewhere, the scientific knowledge of fossil plants primarily depends.

It is scarcely necessary to repeat what has been said so often elsewhere, that the now almost universal recognition of the cryptogamic nature of Calamodendreæ and Sigillariæ is a splendid triumph for the opinions of the late Prof. Williamson, which he gallantly maintained through a quarter of a century of controversy.

Perhaps, however, the keenest interest now centres in the Ferns and fern-like plants of the Carboniferous epoch. No fossil remains of plants are more abundant or more familiar to collectors, than the beautiful and varied fern-fronds from the older strata. The mere form, and even the venation of these fronds, however, really tell us little, for we know how deceptive such characters may be among recent plants. In a certain number of cases, discovery of the fructification has come to our aid, and were sori are found we can have no more doubt as to the specimens belonging to true Ferns. The work of Stur and Zeiller has been especially valuable in this direction, and has revealed the interesting fact that a great many of these early Ferns showed forms of fructification now limited to the small order Marattiaceæ. I think perhaps the predominance of this group has been somewhat exaggerated, but at least there is no doubt that the marattiaceous type was much more important then than now, though it by no means stood alone. In certain cases the whole fern-plant can be built up. Thus Zeiller and Renault have shown that the great stems known as *Psaronius*, the structure of which is perfectly preserved, bore fronds of the *Pecopteris* form, and that similar *Pecopteris* fronds produced the fructification of *Asterotheca*, which is of a marattiaceous character. Hence, for a good many Carboniferous and Permian forms there is not the slightest doubt as to their fern-nature, and we can even form an idea of the particular group of Ferns to which the affinity is closest.

I will say nothing more as to the true Ferns, though they present innumerable points of interest, but will pass on at once to certain forms of even greater importance to the comparative morphologist.

A considerable number of palæozoic plants are now known which present characters intermediate between those of Ferns and Cycadeæ. I say *present intermediate characters*, because that is a safe statement; we cannot go further than this at present, for we do not yet know the reproductive organs of the forms in question.

In *Lyginodendron*, the vegetative organs of which are now completely known, the stem has, on the whole, a cycadean structure; the anatomy, which is preserved with astonishing perfection, presents some remarkable peculiarities, the most striking being that the vascular bundles of the stem have precisely the same arrangement of their elements as is found in the leaves of existing Cycads, but nowhere else among living plants. The roots also, though not unlike those of certain ferns in their primary organisation, grew in thickness by means of Cambian, like those of a Gymnosperm. On the other hand, the leaves of *Lyginodendron* are typical fern-fronds, having the form characteristic of the genus *Sphenopteris*, and being probably identical with the species *S. Hæninghausi*. Their minute structure is also exactly that of a fern-frond, so that no botanist would doubt that he had to do with a Fern if the leaves alone were before him.

This plant thus presents an unmistakable combination of cycadian and fern-like characters. Another and more ancient genus, *Heterangium*, agrees in many details with *Lyginodendron*, but stands nearer the ferns, the stem in its primary structure resembling that of a *Gleichenia*, though it grows in thickness like a cycad. These intermediate characters led Prof. Williamson and myself to the conclusion that these two genera were derived from an ancient stock of Ferns, combining the characters of several of the existing families, and that they had already considerably diverged from this stock in a cycadean direction. I believe that recent investigations, of which I hope we shall hear more from Mr. Seward, tend to supply a link between *Lyginodendron* and the more distinctly cycadean stem known as Cycadoxylon.

Heterangium first appears in the Burntisland beds, at the base of the Carboniferous system; from a similar horizon in Silesia, Count Solms-Laubach has described another fossil, *Perotopitys Bucheana*, the vegetative structure of which also shows, though in a different form, a striking union of the characters of Ferns and Gymnosperms. Count Solms shows that this genus cannot well be included among the Lyginodendreæ, but must be placed in a family of its own, which, to use his own words, "increases the number of extinct types which show a transition between the characters of Filicineæ and of Gymnosperms, and which thus might represent the descendants in different directions of a primitive group common to both." (*Bot. Zeitung*, 1893, p. 207.)

Another intermediate group, quite different from either of the foregoing, is that of the Medulloseæ, fossils most frequent in the Upper Carboniferous and Permian strata. The stems have a remarkably complicated structure, built up of a number of distinct rings of wood and bast, each growing by its own cambium. Whether these rings represent so many separate primary cylinders, like those of an ordinary polystelic Fern, or are entirely the product of anomalous secondary growth, is still an open question, on which we may expect more light from the investigations of Count Solms. In any case, these curious stems (which certainly suggest in themselves some relation to Cycadeæ) are known to have borne the petioles known as *Myeloxylon*, which have precisely the structure of cycadean petioles. (Seward, "Annals of Botany," vol. vii. p. 1.)

Renault has further brought forward convincing evidence that these *Myeloxylon* petioles terminated in distinctly fern-like foliage, referable to the form-genera *Alethopteris* and *Neuropteris*. Hence it is evident that the fronds of these types, like some specimens of *Sphenopteris*, cannot be accepted as true ferns, but may be strongly suspected of belonging to intermediate groups between Ferns and Cycads.

It is not likely (as has been repeatedly pointed out elsewhere) that any of these intermediate forms are really direct ancestors of our existing Cycads, which certainly constitute only a small and insignificant remnant of what was once a great class, derived, as I think the evidence shows, from fern-like ancestors, probably by several lines of descent.

One of the greatest discoveries in fossil botany was undoubtedly that of the Cordaiteæ—a fourth family of Gymnosperms, quite distinct from the three now existing, though having certain points in common with all of them. They are much the most ancient of the four stocks, extending back far into the Devonian. Nearly all the wood of Carboniferous age, formerly referred to Coniferæ under the name of *Dadoxylon* or *Araucarioxylon*, belonged to these plants. Thanks chiefly to the brilliant researches of Renault and Grand' Eury, the structure of these fine trees is now known with great completeness. The roots and stems have a coniferous character, but the latter contain a large, chambered pith different from anything in that order. The great simple lanceolate or spatulate leaves, sometimes a yard long, were traversed by a number of parallel vascular bundles, each of which has the exact structure of a foliar bundle in existing Cycadeæ. This type of vascular bundle is evidently one of the most ancient and persistent of characters. Both the male and female flowers (*Cordaianthus*) are well preserved in some cases. The morphology of the former has not yet been cleared up, but the stamen, consisting of an upright filament bearing 2–4 long pollen-sacs at the top, is quite unlike anything in Cycadeæ; a comparison is possible either with *Gingko* or with the Gnetaceæ.

In the female flowers—small cones—the axillary ovules appear to have two integuments, a character which resembles Gnetaceæ rather than any other Gymnosperms. Renault's famous discovery of the prothallus in the pollen-grains of *Cordaites* indicates the persistence of a cryptogamic character; but it cannot be said that the group as a whole bears the impress of primitive simplicity, though it certainly combines in a remarkable way the characters of the three existing orders of the Gymnosperms.

There is one genus, *Poroxylon*, fully and admirably investigated by Messrs. Bertrand and Renault, which from its perfectly preserved vegetative structure (and at present nothing else is known) appears to occupy an intermediate position between the Lyginodendreæ and the Cordaiteæ. The anatomy of the stem is almost exactly that of *Lyginodendron*, the resemblance extending to the minutest details, while the leaves seem to closely approach those of *Cordaites*. *Poroxylon* is at present known only from the Upper Carboniferous, so we cannot regard it as in any way representing the ancestors of the far more ancient Cordaiteæ. The genus suggests, however, the possibility that the Cordaiteæ and the Cycadeæ (taking the latter term in its wide sense) may have had a common origin among forms belonging to the filicinean stock. It is also possible that the Cordaiteæ, or plants allied to them, may in their turn have given rise to both Coniferæ and Gnetaceæ.

It is unfortunate that at present we do not know the fructification of any of the fossil plants which appear to be intermediate between ferns and Gymnosperms. Sooner or later the discovery will doubtless be made in some of these forms, and most interesting it will be. M. Renault's *Cycadospadix* from Autun appears to show that very cycad-like fructifications already existed in the later Carboniferous period, and numerous isolated seeds point in the same direction, but we do not know to what plants they belonged.

I think we may say that such definite evidence as we already possess decidedly points in the direction of the origin of the Gymnosperms generally from plants of the Fern series rather than from a lycopodiaceous stock.

I must say a few words before concluding on the cycad-like fossils which are so striking a feature of mesozoic rocks, although I feel that this is a subject with which my friend Mr. Seward is far more competent to deal. Both leaves and trunks of an unmistakably cycadean character are exceedingly common in many mesozoic strata, from the Lias up to the Lower Cretaceous. In some cases the structure of the stem is preserved, and then it appears that the anatomy as well as the external morphology is, on the whole, cycadean, though simpler, as regards the course of the vascular bundles, than that of recent representatives of the group.

Strange to say, however, it is only in the rarest cases that fructifications of a truly cycadean type have been found in association with these leaves and stems. In most cases, when the fructification is accurately known, it has turned out to be of a type totally different from that of the true Cycadeæ, and much more highly organised. This is the form of fructification characteristic of *Bennettites*, a most remarkable group, the organisation of which was first revealed by the researches of Carruthers, afterwards extended by those of Solms-Laubach and Lignier. The genus evidently had a great geological range, extending from the Middle Oölite (or perhaps even older strata) to the Lower Greensand. Probably, all botanists are agreed in attributing cycadean affinities to the Bennettiteæ, and no doubt they are justified in this. Yet the cycadean characters are entirely vegetative and anatomical; the fructification is as different as possible from that of any existing cycad, or, for that matter, of any existing Gymnosperm. At present, only the female flower is accurately known, though Count Solms has found some indications of anthers in certain Italian specimens. The fructification of the typical species, *B. Gibsonianus*, which is preserved in marvellous perfection in the classical specimens from the Isle of Wight, terminates a short branch inserted between the leaf-bases, and consists of a fleshy receptacle bearing a great number of seeds seated on a long pedicel with barren scales between them. The whole mass of seeds and intermediate scales is closely packed into a head, and is enclosed by a kind of pericarp formed of coherent scales, and pierced by the micropylar terminations of the erect seeds. Outside the pericarp, again, is an envelope of bracts which have precisely the structure of scale-leaves in cycads. The internal structure of the seeds is perfectly preserved, and strange to say, they are nearly, if not quite, exalbuminous, practically the whole cavity being occupied by a large dicotyledonous embryo.

This extraordinary fructification is entirely different from that of any other known group of plants, recent or fossil, and charac-

terises the Bennettiteæ, as a family perfectly distinct from the Cycadeæ, though probably, as Count Solms-Laubach suggests, having a common origin with them at some remote period. The Bennettiteæ, while approaching Angiosperms in the complexity of their fruit, retain a filicinean character in their ramenta, which are quite like those of ferns, and different from any other form of hair found in recent Cycadeæ. Probably the bennettitean and cycadean series diverged from each other at a point not far removed from the filicinean stock common to both.

I hope that the hasty sketch which I have attempted of some of the indications of descent afforded by modern work on fossil plants may have served to illustrate the importance of the questions involved and to bring home to botanists the fact that phylogenetic problems can no longer be adequately dealt with without taking into account the historical evidence which the rocks afford us.

Before leaving this subject I desire to express the great regret which all botanists must feel at the recent loss of one of the few men in England who have carried on original work in fossil botany. At the last meeting of the Association we had to lament the death, at a ripe old age, of a great leader in this branch of science, Prof. W. C. Williamson. Only a few weeks ago we heard of the premature decease of Thomas Hick, for many years his demonstrator and colleague. Mr. Hick profited by his association with his distinguished chief, and made many valuable original contributions to palæobotany (not to mention other parts of botanical science), among which I may especially recall his work, in conjunction with Mr. Cash, on *Astromyelon* (now known to be the root of Calamites), on the leaves and on the primary structure of the stem in Calamites, on the structure of *Calomostachys*, on the root of *Lyginodendron*, and on a new fossil probably allied to *Stigmaria*. His loss will leave a gap in the too thin ranks of fossil-botanists; but we may hope that the subject, now that its importance is beginning to be appreciated, will be taken up by a new generation of enthusiastic investigators.

CONCLUSION.

To my mind there is a wonderful fascination in the records of the far-distant past in which our own origin, like that of our distant cousins the plants, lies hidden. If any fact is brought home to us by the investigations of modern biology, it is the conviction that all life is one: that, as Nägeli said, the distance from man to the lowest bacterium is less than the distance from the lowest bacterium to non-living matter.

In all studies which bear on the origin and past history of living things there is an element of human interest—

> "Hence, in a season of calm weather,
> Though inland far we be,
> Our souls have sight of that immortal sea
> Which brought us hither."

The problems of descent, though strictly speaking they may often prove insoluble, will never lose their attraction for the scientifically guided imagination.

THE CONWAY EXPEDITION TO SPITZBERGEN.

THE *Times* of September 18 published an account of a conversation which Mr. Trevor-Battye, on his return from his recent journey in Spitzbergen, had with a representative of Reuter's Agency. To this report we are indebted for the following particulars. As will be remembered, Mr. Trevor-Battye was a member of Sir Martin Conway's expedition (an account of the doings of a section of which appeared in NATURE of September 10, from the pen of Dr. J. W. Gregory), and, as arranged, left Sir Martin Conway, Dr. Gregory, and Mr. Garwood, in company with Mr. Conway, the artist, and Pedersen, of Tromsö, near Advent Bay for the purpose of exploring some of the northern parts of the island. The first object was to explore Dickson Bay, the most northerly bay in Ice Fjord, the northern part of which had never been mapped. In this work the explorers seem to have met with very considerable difficulties from flowing ice and the remains of the old winter pack. However, they landed at a place on the western shore, and spent the night. In the morning, the ice having opened a little, Mr. Trevor-Battye and Pedersen crossed to the other side, being anxious to find out something of the character of the country which separates Ice Fjord from the sea lying to the north. At the north end they found the tide was out, and great stretches of mud of a very tenacious character were to be seen. In the distance, running north-west, appeared what seemed to be a valley; but, at a nearer view, it proved not to be a valley at all, but an enormous glacier, the front of which was masked by an immense and intricate moraine. The glacier, in striking contrast to the majority of glaciers, is a retreating one, and is slowly dying back. On reaching it, the explorers found it a mile and a half wide, and many miles in length. Pedersen, being anxious about his boat, returned to her at this stage, and Mr. Trevor-Battye went on alone, and presently climbed the snout of a rounded glacier, by which he hoped to be able to effect a crossing. It was, however, badly crevassed, the crevasses becoming wider and more formidable at every step. In his own words: "I had not expected to find ice, and so was not prepared, not even having a stick or a gun with me. I wanted to push on, however, although aware of the fact that the undertaking was rash, and one which, under the circumstances, no Alpine guide would have attempted. I went some distance further, but, sinking to my knees on a snow-bridge half-spanning a crevasse, I had to reach the other side by flinging myself forward. Later, while standing at the edge of another crevasse, a large body of solid ice, which was jammed between its walls, fell with a roar as I was going to walk across it. A little ahead I could see the col, from which I knew I should have sight of the sea; but I found it impossible to proceed without proper ice tools, for the crevasses between me and that point were masked by deep snow, and I felt any further attempts to be quite unjustifiable. I had now reached a height of 1800 feet—not of mountain, but a gradual rise of ice-river from the sea. The return journey I found more difficult, as the crevasses had to be met down hill, and a slip upon their rounded edges would have been fatal. Finally, I rejoined Pedersen after a walk of twenty-two hours. We then returned to Cape Wärn, and explored the western bay of Ice Fjord. According to Nordenskiöld's map, on which our Admiralty chart is based, a large island occupies the centre of this bay; but, after cruising about for two days, we found to our surprise that it no longer existed as an island. The glacier —which, by the way, we named 'Splendid Glacier'—had encroached to such an extent, and so rapidly, that it had entirely filled up one neck of the bay, and had also covered two-thirds of the island. In a few years' time the head of the bay will be completely obliterated."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

DR. CARL VON KUPFFER, Professor of Anatomy in the University of Munich, has been elected Rector of that University for the coming year.

MR. J. AIREY, of the Leeds Organised Science School, has been appointed science master of the Rhondda Intermediate School at Porth.

MR. F. T. HOWARD, Professor of Geology in Cardiff University College, has been appointed one of her Majesty's inspectors of schools.

MR. H. J. MACKINDER will deliver, at Gresham College, under the auspices of the University Extension Society, a course of twenty-five lectures on "The Geography of Europe, Asia, and Northern Africa," beginning on October 5, at six o'clock.

DR. E. SYMES THOMPSON, Gresham Professor of Medicine, will deliver lectures on "Vaccination," on October 6 and 7, and on "The New Photography," on October 8 and 9. The lectures, for which no charge for admission is made, will be given at six o'clock each evening in Gresham College, Basinghall Street, E.C.

THE Councils of University College and of King's College, London, have, in conjunction with the Technical Education Board of the London County Council, arranged a number of courses to be held in the evenings for those students who are engaged in the day-time. The courses are to be of the same standard as the day classes, and admission will be confined to students who have already made some advance in the knowledge of the subjects. At University College there will be lectures on mechanical engineering, by Prof. Hudson Beare, commencing October 12; electrical engineering, by Prof. Fleming, commencing October 13; and practical chemistry, by Mr. C. F. Cross, commencing November 1. At King's College the special evening classes for advanced students are: Civil

engineering, by Prof. Robinson, beginning Monday, October 5; architecture, by Prof. Banister Fletcher, beginning on Wednesday, October 7; experimental and practical physics, beginning on Monday, October 5, under the direction of Prof. Adams, F.R.S.; pure mathematics—higher mathematics—by Prof. Hudson, beginning on Tuesday, October 6; and a free Saturday morning class for elementary teachers—strength of materials and theory of machines—by Prof. Capper, beginning Saturday, October 17. Application to join any of the above classes should be made, as soon as possible, to the Professors who will conduct the courses. The formation of the classes is a new feature of the work of the London Technical Education Board, and it is one which will advance technical education in the right direction.

SCIENTIFIC SERIALS.

Wiedemann's Annalen der Physik und Chemie, No. 9.—Effect of light on spark discharges, by E. Warburg. This effect is not a direct action, but is the consequence of the shortening of a process preceding the spark discharge, and this shortening is brought about by illumination. The author studied the shortening by applying the difference of potential more or less rapidly, and finding the lowest difference of potential capable of producing discharge within five minutes, this being the greatest delay observed. The discharge potential thus found he calls the statical discharge potential, to distinguish it from the dynamic discharge potential producing sparks when the current surges to and fro. The experiments made by the author show that the statical discharge potential is not materially influenced by illumination. But when a difference of potential nearly seven times as high is applied for a few thousandths of a second only, it always produces discharge when the kathode is illuminated by an arc lamp, and not in the dark. The range of potentials at which discharge only takes place occasionally is very small in the case of illumination, but large in the dark. This explains why a telephone connected with an illuminated spark gap gives a purer note than when it is not illuminated.—Electric refractive indices of water and aqueous solutions, by P. Drude. For oscillations of the frequency of 4×10^8 per second the square of the electric index of refraction at 17° C. is 81·67. Water possesses slight normal dispersion, since the square is 80·60 for a frequency of $1·5 \times 10^8$, and 83·6 for 8×10^8. Between 0° and 26° the change of n^2 is proportional to the temperature. It decreases by 0·367 per degree. At higher temperatures the decrease is slower. The refractive indices of dilute aqueous solutions are very nearly the same as those of water.—Dilute ferromagnetic amalgams, by H. Nagaoka. In fields of less than 20 C.G.S. units the magnetisation of iron amalgams shows a discontinuity at the melting-point. On heating an amalgam containing 1·78 per cent. of iron, produced by electrolysis, up to its melting-point (−38° C.), the intensity of magnetisation in a field of 16 units gradually increased. It suddenly attained a maximum on melting, and gradually diminished on further heating.—Influence of pulling and pushing forces upon magnetic properties, by G. S. Meyer. Cobalt also shows the effect discovered in iron by Villari of a maximum of magnetic intensity when under a certain force. In nickel and cobalt tension produces an E.M.F. identical in direction with that produced by longitudinal magnetisation.—An attempt to liquefy helium, by K. Olszewski. (See p. 377.) Helium cannot be liquefied by the most powerful methods yet available. It is more permanent than hydrogen, probably owing to its monatomic structure, and is on that account valuable as a thermometric substance at very low temperatures. A comparison of a helium and a hydrogen thermometer shows, however, that hydrogen has normal expansion as far as −234·5° C., its critical temperature, and is therefore available for thermometric use down to that point.

Bollettino della Società Sismologica Italiana, vol. ii., 1896, No. 3.—On the Benevento earthquake of March 14, 1702, by M. Baratta. A discussion of the earthquake founded on three old documents recently discovered, and of its relations to the Benevento earthquakes of June 1688 and September 1885.—Present state of the endogenous phenomena in the Eolian islands, by A. Riccò.—Considerations on recording seismic apparatus and modification of the two-component microseismograph, by G. Vicentini and G. Pacher. A reprint of a paper already noticed in NATURE.—Summary of the principal eruptive phenomena in Sicily and the adjacent islands during the six months January to June, 1896, by S. Arcidiacono.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 21.—M. A. Cornu in the chair.—The President announced the loss that the Academy had sustained in the death of M. Fizeau, and the meeting was adjourned in consequence.

NEW SOUTH WALES.

Linnean Society, July 29.—Mr. Henry Deane, President, in the chair.—Appendix to the Australian *Clivinides* (fam. *Carabidæ*), by Thomas G. Sloane. Since his paper was read (at the June meeting) the author has had the opportunity of examining the *Clivinides* of King's Sound, W.A., and its vicinity, in the Macleay Museum. The collection comprises sixteen species, of which two are described as new.—Descriptions of new species of Australian Coleoptera, by Arthur M. Lea. Two genera and thirty-four species belonging to the families *Tenebrionidæ* and *Curculionidæ* are described as new. Two very interesting species are noted—an apterous *Pterohelæus* and a Cossonid having an 8-jointed funicle.—Descriptions of some new *Araneidæ* of New South Wales, No. 6, by W. J. Rainbow. Eight species, comprising representatives of the genera *Nephila*, *Epeira*, *Dolomedes*, and *Actinoups*, are described as new. The last named is specially interesting from the fact that it is the first of the genus recorded from Australia. Five of the spiders described are remarkable for their protective colouration or mimicry; in addition to these, numerous other examples are instanced. After summing up all the facts recorded, the writer concludes by dividing the *Araneidæ* into two groups, viz.: (1) (*a*) spiders whose colouration and (*b*) formation is protective: and (2) spiders that mimick, (*a*) animate or (*b*) inanimate objects, and (*c*) whose colours are attractive.—Description of a new species of *Ablepharus* from Victoria, with critical remarks on two other Australian lizards, by A. H. S. Lucas and C. Frost. *Ablepharus rhodonoides*, sp.n., from Mildura, is allied to *A. greyi*, Gray, by the head-scaling, but in habit it resembles species like *A. muelleri*, Fischer, and *A. lineatus*, Bell, which are remarkable for the reduction in the size of the limbs, as well as in the number of the digits. *A. greyi*, described from West Australia, is recorded from the Boggabri District, N.S.W. *Hemisphæriodon tasmanicum*, Lucas and Frost (Proceedings, 1893, p. 227), as the outcome of the examination of series of additional specimens, is now reduced to a variety of the very variable *Homolepida casuarineæ*, D. and B.—On a new genus and three new species of mollusca from New South Wales, New Hebrides, and Western Australia, by John Brazier.

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